

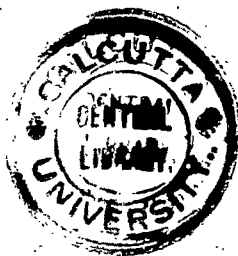
Cole -H04053-1-P015608

ANTIQUITY

A Quarterly Review of Archaeology



Edited by O. G. S. Crawford, F.B.A.



570.5
A.1234

VOLUME XXIII

1949

P15608

Printed by
JOHN BELLOWS LTD.
GLOUCESTER

Contents, Volume XXIII

No. 89, March 1949

	PAGE
Editorial Notes	I
Romano-Buddhist Art: an old problem restated. By R. E. M. WHEELER	4
The Dumb and the Stammerers in Early Irish History. By HUBERT BUTLER	20
The Excavations at Cairnpapple Hill, West Lothian 1947-8. By STUART PIGGOTT	32
Roots and Origins: a review. By E. G. R. TAYLOR	40
NOTES AND NEWS:	
Cave-man in France. By Hallam L. Movius, Jr.	44
Excavation of a Cheviot Hill-fort	45
Urn-fields and Vital Statistics. By Grahame Clark	46
Recent Discoveries in Azarbaijan. By T. Burton Brown	47
Arthur's Battle of Breguoin. By Kenneth Jackson	48

No. 90, June 1949

Editorial Notes	57
'Buried Landscapes' in Southern Italy. By JOHN BRADFORD	58
Problems and Policies. By F. T. WAINWRIGHT	73
The taking of Le Krak des Chevaliers in 1271. By D. J. CATHCART KING	83
Kingship and the Gods: a review. By M. E. L. MALLOWAN	93
Men, Machines and History. By O. G. S. CRAWFORD	100
Important New Books and Articles	107

No. 91, September 1949

Editorial Notes	113
Ninety Years Ago. By JOAN EVANS	115
Human Evolution: a review. By E. A. HOOTON	126
The Origin of Neolithic Culture in Northern Europe. By V. G. CHILDE	129
Peru before Pizarro. By G. H. S. BUSHNELL	136
Customs and Traditions of the Isle of Portland, Dorset. By ROBERT DOUCH	140
Canterbury Excavations, 1944-8. By SHEPPARD FRERE	153
Roman Coins found in Iceland. By HAAKON SHETELIG	161
Important New Books and Articles	164

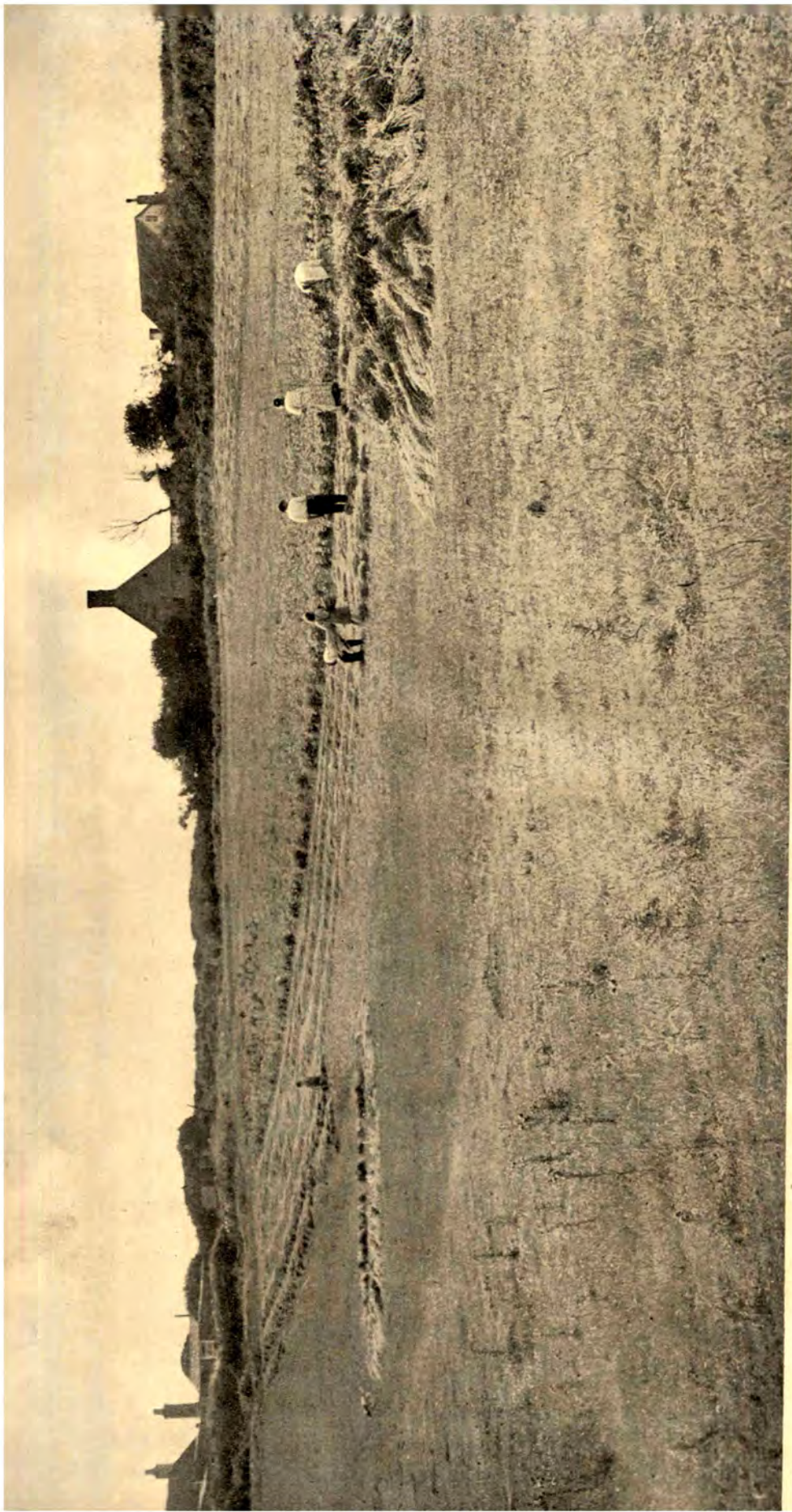
CONTENTS

No. 92, December 1949

	PAGE
Editorial Notes	169
Moas and Man (part. I). By R. S. DUFF	172
The Open Field in Devonshire. By H. P. R. FINBERG	180
The Restoration of the Monastery Church of Debra Damo, Ethiopia. By DEREK H. MATTHEWS	188
A second fixed point in the Chronology of the Harappa Culture. By J. F. S. STONE	201
Important New Books and Articles	206
NOTES AND NEWS :—	
A Mesolithic Habitation-site in Yorkshire	207
Excavations on Blewburton Hill, Berks	208
Earthwork at Old Yevering, Northumberland. By A. H. A. Hogg	211
Sixth International Congress for the History of Science	214
'Vases Supports'. By J. F. S. Stone	215
Excavations at Whithorn	217
Long Barrow near Pitsford, Northants. By Joy Fereday	218
Pins with Animal or Spiral Heads. By R. W. Hutchinson	220

REVIEWS OF BOOKS

<i>Annual of the American Schools of Oriental Research</i>	168
<i>Archaeology</i>	51
Chapple (E. D.) and Coon (C. S.). <i>Principles of Anthropology</i>	54
Crawford (O. G. S.). <i>Topography of Roman Scotland</i>	108
Diringer (David). <i>The Alphabet</i>	108
Edwards (I. E. S.). <i>The Pyramids of Egypt</i>	225
Eisler (Robert). <i>The Royal Art of Astrology</i>	223
Ellis (Colin D. B.). <i>History in Leicester</i>	112
Emery (Walter B.). <i>Nubian Treasure</i>	50
Finegan (Jack). <i>Light from the Ancient Past</i>	221
Gordon (D. H.). <i>Early Indian Painted Pottery</i>	227
Latham (Ronald). <i>In Quest of Civilization</i>	221
Mackay (Ernest). <i>Early Indus Civilizations</i>	166
Masai (F.). <i>Essai sur l'Origines de la Miniature dite Irlandaise</i>	110
<i>Ordnance large and small scale maps</i>	222
Palmer (G. Blake). <i>New Zealand Archaeology and Air-photography</i>	223
Piggott (Stuart). <i>Some Ancient Cities of India</i>	168
Ramskou (Th.). <i>Himmerlands Oldtidsminder</i>	53
Rose (H. J.). <i>Aeneas Pontifex</i>	53
Saxl (F.) and Wittkower (R.). <i>British Art and the Mediterranean</i>	165
Tarn (W. W.). <i>Alexander the Great</i>	227
Tilly (Bertha). <i>Vergil's Latium</i>	228
Watkin (Dom Aelred). <i>The Great Chartulary of Glastonbury</i>	224
Williamson (Kenneth). <i>The Atlantic Islands</i>	53
Wittkower (R.). See Saxl (F.)	165



MEDIEVAL HUSBANDRY IN SARK
Ph. M. T. Myres

Antiquity

VOL. XXIII No. 89

MARCH 1949

Editorial Notes

THAT unnecessary war which breaks out from time to time between the amateur and the professional has flared up again in the columns of a contemporary. It began with an article published by Professor Stuart Piggott, who though now securely entrenched in the ranks of the professional 'army', began like most of us as an amateur. The moral is of course obvious, the more so as archaeology has long been a profession where vacancies are hard to fill.

It is difficult to discover what the fuss is all about. Some of those who are most vocal seem to imagine that professors want to suppress amateurs or monopolize opinion; the truth is that whole-time students are much too deeply immersed in masses of facts to bother much about mere opinions, which too often take the form of vague theorizing unsupported by evidence. Anyone of course is free to express an opinion; its value depends upon the factual knowledge and experience of the person who expresses it. Opinions about the meaning of place-names, for example, are valueless if the early forms are unknown; such opinions, when expressed as articles, are rejected, not because editors dislike amateurs—they don't—but because the writers have not taken the trouble to learn their job. In such cases the remedy lies with the amateur; and it is observed that the most aggressive are those who have contributed least to knowledge. Not only is there nothing (except economic difficulties) to prevent an amateur from practising and eventually becoming a professional, if he wishes; but it is also true that there is nothing that gives the professional (whether as professor, editor or civil servant) such pleasure as to come across an amateur who really means business and can be helped.

Given the necessary leisure—and where there's a will there's always a way to find it—the amateur can always somehow familiarize himself with the facts of archaeology, or history, or geology, or whatever subject it is that interests him. And unless he can so familiarize himself, he will never become an archaeologist in any real sense. He cannot become one simply by reading books, going to lectures or even by attending the meetings of archaeological societies, though these all help, and especially the human contacts of the last. Nor do people become archaeologists by writing books about the lost continent of Atlantis or the Children of the Sun or Downland Man, but by walking

ANTIQUITY

about the country with a map and an observant eye. Nor do they become archaeologists by learning a dead language; it is much more necessary to be able to draw plans and sections, to take photographs, to give first aid to fragile objects and to understand the soil. It is not the professional archaeologist who prevents the amateur from acquiring such qualifications as these.

Modern archaeology began with Boucher de Perthes, Schliemann and Pitt-Rivers. Boucher de Perthes was the first to reason from objects to logical conclusions (and not to books); what he found was not explained by quotations from the classics; it explained itself—and the antiquity of man. Schliemann was the first to dig for knowledge, not museum specimens; his technique was as good as he could make it, and (as his notes show) much better than his books would suggest. Pitt-Rivers created the modern technique upon a secure basis of military precision and discipline. All were amateurs; there were then no professionals outside museums. Boucher de Perthes was the typical local antiquary, Pitt-Rivers the typical country gentleman. Those heroic days have passed; the success of the pioneers was due to a combination of personal qualities with concentration on a chosen subject maintained by enthusiasm and will-power. Again no professional archaeologist stands in the way of the amateur who wishes to succeed.

It is difficult to give any advice that will be of much constructive use in a particular case; so much depends upon the exact conditions. Generally however it is best to proceed from the particular to the general, from the known to the unknown; to try to relate the particular enquiry to some matter of general interest. To be able to do this demands some knowledge of theory, of what archaeology aims at. Such background knowledge *can* be got from books (including the back numbers of ANTIQUITY). Once acquired it enables one to see things that otherwise would be overlooked. One learns, for example, that the important things in human history are the common things—houses, tools, roads, pottery, domestic animals and plants, simple forms of art and decoration. A traveller in the Balkans or Arabia will come across houses built of mixed rubble and timber, and will remember, perhaps, the similar construction (called *opus Gallicum*) of some Scottish, French and Swiss hill-forts, petrified also in the obelisks of Axum (see ANTIQUITY xx, 4-8; 60-69). Or he may observe some primitive type of boat, perhaps one that even Mr Hornell has missed, or unrecorded varieties of domestic animals. By photographing them (especially close up and in detail) he will be doing a useful piece of work; such photographs are seldom taken and much in demand. Editors and publishers are often glad to use such illustrations (when they are good enough). There is no 'closed shop' here.

At home there is field-work. The scope is unlimited, and the material equipment an Ordnance Map on the scale of 2½ or 6 inches to the mile. There are many miles of Roman roads still to be discovered and put on the map; the best and only training is to walk along a good stretch of *known* road, study its appearance and habits, consult some of the standard articles and books (such as Mr Margary's on the roads of the Weald), and then try and fill one of the many gaps on the Ordnance Map of Roman Britain. Once

EDITORIAL NOTES

one has become familiar with the signs one can find Roman roads to test on the ground merely from a study of the 6-inch Ordnance Map, especially in Wales, where even on the map one notes some earthworks that are probably unrecognized Roman forts. A very little experience shows where such forts are likely to be situated. In the field many unexpected minor discoveries can be made. If the amateur archaeologist prefers navy work on a dig to this exciting pastime, the professional will hardly try and dissuade him, but the choice is open ; there is no ' direction ' of his labour.

Let it not for a moment be imagined that the professional despises the amateur, or wishes to keep him out. If the amateur has the root of the matter in him the professional will welcome him with open arms and do all he can to help him (though this is not always easy) ; but the amateur must do his own thinking. Certain techniques can only be learnt in the orthodox way, by undergoing a course of training ; field-work can only be learnt by doing it. Baiting professors may be amusing and good for the circulation ; but to be effective it must be factual and realistic.

The picture which we publish as a frontispiece was taken last August in the island of Sark by Mr Timothy Myres. We publish it partly because it is a very good illustration of a purely medieval type of arable husbandry, partly because it is also a very charming picture. There are probably few places in northern Europe where this kind of work still goes on, and one who saw the scene ' had a feeling that this was taking place round about the Norman Conquest, and that I could have got on terms at once with the harvesters had I spoken to them in the Norseman's French which their *patois* recalls '. As can be seen, the field is divided into strips, the two nearest being fallow ; on the second are curious circular ricks with hollow centres to which the children are dragging the sheaves of oats that are being harvested on the third. The whole family is engaged on the job—two men, two women, and several children (not to mention the dog, who appears to have important business to attend to elsewhere). The oats had been sown broadcast and cut with a scythe. When the picture was taken, the four grown-ups were making their second turn up the full length of the strip, gathering the swathes and binding them by hand into sheaves. The scene brings to life again the activities that must have gone on every year in every agricultural village in England, and is a nice illustration complementary to recent notes in *ANTIQUITY* about strip-cultivation.

We would remind our readers that subscriptions for 1949 are now due and should be sent (together with the enclosed form) to *ANTIQUITY*, The Wharf, Newbury, Berks. This address, and not the Gloucester one, is now the one to which all such correspondence should be sent. An early payment saves labour and is much appreciated. Forms have been inserted only in the copies of those subscribers who have NOT yet paid.

Romano-Buddhist Art : an old problem restated

by R. E. M. WHEELER

SINCE about 1870, when a Dr Leitner, of the Punjab service, brought from north-western India a small collection of Buddhist 'Indo-Scythian' sculptures to England, the literature of the so-called Gandhāra art has not ceased to grow, and the last decade has added rather more than its quota. Dr H. Buchtal has re-emphasized and re-illustrated the affinities between this Buddhist art and that of the Roman Empire (1). Dr L. Bachhofer has recognized the emergence of a Partho-Buddhist art based on secular Hellenistic imports into Gandhāra in the 1st century A.D. (2) Entrenched behind the formidable ramparts of his unpublished Taxila, that great veteran Sir John Marshall has machine-gunned both the learned doctors with a vigour most happily unimpaired by the ills to which he lightly refers (3). And, since I do not always find myself in agreement with Sir John, I may at once say this : that, had Drs Buchtal and Bachhofer done no more than draw his fire, they would have deserved sufficiently well of us.

To-day the problems of Gandhāra or (to retain the established generic name for the moment) Graeco-Buddhist art have reached a stage at which nothing short of fresh and impeccable material evidence can advance their solution. Reserving Taxila until its explorer's long-awaited report appears, we may affirm that not a single Buddhist site in India or Afghanistan has been excavated with any approach to modern method. Indeed it is not too much to say that, if during the past generation an obscure Roman villa in England had been dug into by a local antiquarian society with the disregard for detail that has consistently marked the excavation of monumental Buddhist sites in India during the same period, our Ancient Monuments Department would have intervened by telegram. The result of this spoliation has been the accumulation of a mass of highly intriguing but undocumented sculpture ranging through almost every shade of style from that of a substantial Sophocles or Apollo Belvedere to an almost wholly Indianized abstraction. The general problem has been further complicated during the last twenty years by the attempted division of this accumulation into two schools separated alike by time and medium. In the circumstances, it has seemed to me that, even in the dearth of new data, we may profitably employ ourselves for a while in restating the problem as it stands at the present moment, prior to handing it over once more with our hopeful blessing to some skilled fieldworker.

* * * * *

By way of preface, it may be recalled that, with the very doubtful exceptions of certain stūpa-mounds at Lauriyā-Nandangarh in northern Bihar and Piprahwa in eastern U.P., there are no material relics of Buddhism that can be ascribed to a period prior to the great Aśoka (c.273-232 B.C.). Thereafter a mass of architectural, structural

¹ 'The Common Classical Sources of Buddhist and Christian Narrative Art', *Journ. Roy. As. Soc.* 1943, pp. 137 ff; 'The Western aspects of Gandhāra Sculpture', *Proc. Brit. Academy*, LXXXI (1945), 3 ff.

² 'On Greeks and Śakas in India', *Journ. Amer. Or. Soc.* LXI (1941), 223 ff.

³ *Journ. Roy. As. Soc.* 1946, pp. 116 ff.; *ibid.* 1947, pp. 3 ff.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

and pictorial evidence bears witness to a variety of evolving ideas and schools which it is convenient, though not strictly correct, to classify broadly under the two main divisions of Buddhist observance; the Hīnayāna or Lesser Vehicle of Salvation, and the Mahāyāna or Greater Vehicle. This is not the context in which to consider in any detail the differences between the two observances, but the broad distinction is of relevant importance and was briefly this: that Hīnayāna Buddhism was primarily a moral philosophy not altogether dissimilar to the Stoicism of the West, whereas the Buddhism associated (though not exclusively) with the Mahāyāna observance was a religion. In the former the Buddha was an inspired teacher who preached the Middle Path between indulgence and asceticism and adopted the traditional Indian belief in rebirth, seeking an ultimate deliverance from accumulated sin in supreme detachment, *nirvāṇa*. The Buddha was not a god, and the earlier Buddhism was a way of life, not a religion. By a process of evolution, however, natural to a country where reverence for the teacher is deep-seated, the Buddha gradually assumed the stature of a god to whom prayer might properly be offered, and this process received formal recognition in or about the 2nd century A.D., when the Mahāyāna persuasion became the dominant mode.

For our present purpose, the outstanding difference between the two types of Buddhism was that during the prevalence of the Hīnayāna teaching the Buddha himself was never represented in art. His presence was symbolized by a chair, a footprint, an umbrella, a riderless horse. About this symbol crowd in tumultuous masses the other participants in the scene; but there is no central commanding figure. In Mahāyāna Buddhism, on the other hand, the figure of the divine Buddha controls the assembly and is the focus of its composition. Both iconographically and aesthetically, the change was revolutionary.

In the present paper we are concerned only with the sculpture of this later Buddhist phase, and we are justified on more than one account in abstracting it from its environment. Although, as has often enough been pointed out, there is no integral 'Buddhist period' in Indian history, it cannot be denied that during the seven centuries between 250 B.C. and A.D. 450 most of the surviving sculpture of the highest quality in India was associated with Buddhism; and it was, above all, Buddhism that during the same period (and particularly the latter part of it) spread Indian art and idiom through the highways and byways of Asia. Archaeologically at least we cannot treat Buddhism merely as a heresy against a prevailing and fundamental Brāhmanical orthodoxy, however little its tenets may have affected the routine of village-life.

* * * * *

Here we are concerned with the art which centres round the Buddha not merely as a Presence but as a Person. The earliest undisputed personification of the Buddha occurs on certain coins of the Kushāna king Kanishka, whose accession to a kingdom based on Gandhāra (the Vale of Peshawar and its environs) has been variously dated between A.D. 78 and 144. The latter date is that calculated recently by R. Ghirshman, ingeniously arguing backwards from the incompletely published epigraphical evidence found in 1939 at Naksh-i-Rustam for the overthrow of the Kushānas by Shapur I between A.D. 241 and 251 (4). Computing in the reverse direction, Sir John Marshall has suggested A.D. 128 for the accession (5); and on other grounds also it may be affirmed with

⁴ *Journ. Asiatique* CCXXXIV (1943-45), 63 ff.; and *Bégram, recherches archéologiques et historiques sur les Kouchans* (Cairo, Institut Français d'Archéologie Orientale, 1946), pp. 99 ff.

⁵ *Journ. Roy. As. Soc.*, 1947, p. 32.

ANTIQUITY

reasonable assurance that Kanishka came to the throne sometime during the second quarter of the 2nd century A.D. and was ruling an expanding kingdom in the middle of that century. Thus the earliest known representation of the Buddha may be ascribed to within a few years of A.D. 150.

This dating is important and it is worth while to pause for a moment to consider alternative postulates.

Of these the most important is the supposed representation of the Buddha on a coin of Maues (prior to c. 58 B.C.). Against Longworth Dames and Tarn, who believe that the Buddha is in fact intended on this coin, are ranged Bachhofer and Marshall who believe that both this and a similar coin of Azes (second half of 1st century B.C.) represent the ruler himself, squatting with a sword across his knee (6). At the best, the coin is far too uncertain a document wherewith to fill a vacuum of two centuries. An attempt has indeed been made to fill this vacuum by placing in it the celebrated casket (in the British Museum) which bears a figure of the Buddha and was found over a century ago in a stūpa at Bīmārān in Afghanistan (7). This attempt, however, has been discounted by Tarn himself, and with much reason. True, the casket is said to have been associated with four copper coins of Azes (I or II ?); but anyone familiar with the miscellaneous character of stūpa-deposits will appreciate that these coins give, not a date, but a *terminus post quem*. For instance, a stūpa at Manikyālā near Rawalpindi contained seven Roman Republican denarii of the 1st century B.C. with Kushānā coins of the 2nd century A.D. (8).

Likewise to be rejected is the supposed evidence of 'dated' statues of the Buddha (9), since in no instance do we know the era intended in the epigraph. For example, the celebrated Loryān Tangāi Buddha in the Indian Museum at Calcutta, dated '318' of some unspecified era, has been ascribed variously to A.D. 6, 234 and 260, and is likely enough to be of none of these dates (10).

On the other side, we must equally reject the famous Peshāwar casket with its crowning Buddha, although it bears the name and portrait-figure of 'Kanishka', since its iconography suggests that a later Kanishka than the striker of the significant Buddha coins is intended (11). At present, these coins are unsupported, but their evidence at least is beyond reproach. Be it repeated that they establish the use of the Buddha image by c. A.D. 150, at a time when a great part of northern India and eastern Afghanistan was being unified under the strong rule of the first Kanishka.

* * * * *

That the development of Mahāyāna Buddhism and the equivalent sects in north-western India, and of the remarkable art there evolved to express it, was due to the powerful patronage of this ambitious king is in itself sufficiently likely. The emergence of the Buddha as a divine figure, with all that this implied in the co-ordination and

⁶ L. Bachhofer, *Journ. Amer. Or. Soc.*, LXI (1941), 229-30; J. Marshall, *Journ. Roy. As. Soc.*, 1947, p. 14.

⁷ L. Bachhofer, *Early Indian Sculpture* (New York, 1929), II, pl. 140; H. H. Wilson, *Ariana Antiqua* (London, 1841), p. 71; W. W. Tarn, *The Greeks in Bactria and India* (Cambridge, 1938), p. 399.

⁸ A. Cunningham, *Arch. Sur. Ind. Reports* II (Simla, 1871), 162.

⁹ 'There is nothing to be made out of dated Buddha statues'. Tarn, *op. cit.*, p. 300.

¹⁰ *Corpus Inscriptionum Indicarum*, II, pt. 1, p. 106; N. C. Majumdar, *Guide to the Sculptures in the Indian Museum*, pt. II (Delhi, 1937), pp. 18 ff.

¹¹ Majumdar, *op. cit.*, p. 13; Marshall, *Journ. Roy. As. Soc.*, 1947, p. 31.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

centralization of artistic composition, created a need which was essentially novel to India, but was traditional in the classical art of the West. Given the opportunity, it was not unnaturally in the West therefore that the artists of the Kushāna court found the new elements of design, and, with them, some of the actual modes and details which combined to inform their native art with an orderliness and rhythm appropriate to its new function. The resultant mingling of East and West yielded a composite art which can rarely be mistaken for a Western product but nearly always includes recognizably Western features; on the one hand the simulation of Western drapery, the use of Western cupids, swags and other ornament, the adaptation of Roman Imperial types and scenes to the Buddha legend (12); and on the other hand the smooth reflection of a passive introspection that is entirely of the East. The 'extrovert' West and the 'introvert' East are thus paradoxically but skilfully combined. The spiritual content of this art remains oriental, its formulae are frequently and sometimes startlingly occidental.

A few examples will sufficiently illustrate this duality. As the more remote and unexpected element of the two, the Western influence requires demonstration, and a few clear instances may be cited. PLATE I A represents the head of the Apollo Belvedere in the Vatican. PLATE I B is a small terracotta head from northwestern India in the Eric Dickinson collection (13). The identity of the two, not merely in facial feature but in the distinctive tufted coiffure, is self-evident. The Indian head is derived from that of the Apollo, with the characteristically oriental addition of the honorific garland (the *hāra*). Again, PLATE VIII A illustrates part of a stucco sculpture from the Buddhist site at Hadda, Afghanistan; and the resemblance to the Hadrianic Antinous type (2nd century A.D.), represented for example by the well-known Villa Albani relief (14), is unmistakable. The fleshy, effeminate Antinous is at home in the oriental setting but is readily distinguished from the rendering of these traits in native Indian art, although the eastern *hāra* is again added. The stucco heads of the child and satyr, both from Taxila (PLATE III), are pure Graeco-Roman in type and quite un-Indian in inspiration; and the former at least suggests the actual handiwork of a Western artist or the use of a Western mould. The same remark might be offered in regard to the remarkably fine stucco head of a boy, also from Taxila (PLATE II B), which irresistibly recalls that of the young Marcus Aurelius (mid 2nd century A.D.) in the Capitoline Museum at Rome (PLATE II A). Also to the 2nd century (latter half) Dr Allan ascribes a remarkable Indian rendering in Gandhāra stone of the story of the Wooden Horse of Troy, reminiscent of the *tabulae Iliacae* of the early Imperial period in the West (15). Another stone frieze (PLATE V A)—one of many of its class—from Taxila likewise betrays oriental handiwork but is essentially Western in motif. And innumerable stucco heads of ethnological types from Hadda and northwestern India (e.g. PLATE IV B), are difficult to consider apart from such Western renderings as that of the Gaul in the National Museum at Rome (PLATE IV A).

Examples could be multiplied, but these must suffice to demonstrate the use of Roman (Graeco-Roman) prototypes in the Buddhist figure-sculpture. Nor was this relationship confined to detail. The new Buddhist ideology of the Mahāyāna persuasion, with its insistence on the Presence, created, as has already been indicated, the need for a

¹² See, for examples, H. Buchta, *The Western Aspects of Gandhāra Sculpture* (Brit. Academy, 1945); and *Journ. Roy. As. Soc.*, 1943, pp. 137 ff.

¹³ Here illustrated by kind permission of Mr Dickinson, who has generously allowed me to anticipate his own publication.

¹⁴ Eg. E. A. Gardner, *Handbook of Greek Sculpture* (London, 1905), p. 518, fig. 130.

¹⁵ J. Allan, 'A *tabula Iliaca* from Gandhāra', *Journ. of Hellenic Studies* LXVI (1946), 21 ff.

ANTIQUITY

new centralized grouping for which the Buddhist art of the previous phase was not prepared. The result was the transcription of whole elements of composition from Roman Imperial art, in which the dominant personality of the emperor had created a similar problem. Christian art, developing in this respect along parallel lines, found an equivalent solution ; and there can be no doubt that, as Buchtal has demonstrated (16), the ceremonial arrival or departure of the Emperor, represented, for example, on Roman medallions of the 2nd and 3rd centuries, is aesthetically a forebear of the departure of the Buddha from Kapilavāstu or the entry of Christ into Jerusalem. A similar kinship between East and West can be observed in the depiction of other incidents of the Buddha's life (17).

There is then, both in detail and in composition, a substantial link between the Buddhist art current on the northwestern border of India in the 2nd and following centuries A.D. and the art of the classical West. Dispute has arisen as to the labelling of that Western art : as to whether it should more properly be called ' Roman ' or ' Hellenistic ' (18). The dispute is an unreal one, and is linked up with old controversies long dead. ' Roman art ' is generically the classical or semi-classical art of the world which was dominated by Rome ; it varied specifically with varying local skills, temperaments and traditions ; its main descent was from the art of the Hellenistic era, but to extend the term ' Hellenistic ' into the Middle Empire is to confuse and impoverish our terminology. The Western art with which we are now concerned was a phase of the art of the Roman Empire. It was therefore *Roman* in any reasonable usage of the term, and the related Buddhist art may properly be called (as Vincent Smith long ago in fact called it (19)) *Romano-Buddhist* art.

In discussing this composite art, a Western writer is inevitably subject to the suspicion of an over-emphasis of the Western element. At the same time, the oriental critic is liable to be no less partisan in an opposite sense, and it is essential to preserve a proper balance between the two. There is in fact no need for controversy or for nationalist feeling in the matter : we need no more deny or resent the Western influence in Buddhist art than we need deny or resent the Oriental influence in a Paisley shawl. The fact remains and is of great interest, but need not be exaggerated. Having found initial expression with some help from a mature foreign art, Buddhism proceeded to develop its own aesthetic forms and to transmute its borrowings into its own language. The precise course of this development cannot yet be traced in detail, since the chronology of Romano-Buddhist art has not yet been established on an objective basis. But there are slight indications which are worthy of record.

* * * * *

Before we consider these indications, there is another intrusive complication which we must remove from our path. Most of the examples of Romano-Buddhist art which I have cited are of stucco. On the other hand, although the existence of works in stucco was known from the first introduction of this art to the modern world, it was the allied stone sculpture that long commanded the prior attention of the critic. The

¹⁶ H. Buchtal, ' The Western Aspects of Gandhāra Sculpture ', *Proc. Brit. Acad.* xxi (London, 1945).

¹⁷ Ibid.

¹⁸ Marshall, *Guide to Taxila* (3rd ed., Delhi, 1936), p. 33 ; *Journ. Roy. As. Soc.*, 1946, pp. 116 ff.

¹⁹ *Journ. As. Soc. of Bengal*, 1889, p. 172.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

reason for this partiality is simple enough. In and around the Vale of Peshāwar, and particularly in the Swat valley, occurs a green schist which was extensively utilized by the Buddhist sculptors and has necessarily a far greater expectation of survival than the less durable stucco. Furthermore, it was precisely in this region—the ancient Gandhāra—that the British forces in India were concentrated for three generations, and it was inevitable that they should constitute a willing if uncritical agency for the transmission of these easily portable Westernizing works to the collections of India and Europe. The Art of Gandhāra, as it was called, thus became synonymous with the Romano-Buddhist schist sculptures of the Peshāwar region, and the term still tends to monopolize discussion of the subject.

But in the twenties of the present century our perspective began to alter. The excavations carried out by the Archaeological Survey of India at Taxila, on the border of the Punjab and the Northwest Frontier Province, and by the French Archaeological Mission to Afghanistan at Hadda, near Jalālābād, a hundred miles from Peshāwar, revealed large quantities of stucco and some clay sculpture which had to be adjusted to the Gandhāra problem. At about the same time, Sir Aurel Stein was recording kindred stucco-work far away on the trans-Asiatic 'silk-routes' in Chinese Turkestan. Viewed in bulk, this stucco sculpture displayed, as its facile medium might lead us to expect, a somewhat greater range and vivacity than the Gandhāra stone sculpture. Stucco, again, by the use of moulds and the plasticity of its material, lent itself more readily than stone to mass-production, and probably had a more enduring vogue; whilst this same mass-production may in turn have been a contributory factor in the seeming preference of stucco-sculpture for isolated figures or restricted groups rather than for the elaborate *jātaka* and other scenes which the stone-sculptors of Gandhāra were fond of producing. On the other hand, in comparing the two it is necessary always to bear in mind the more fragmentary nature of the surviving evidence in respect of stucco.

What was the relationship of this widespread stucco-work to the more restricted Gandhāra stonework? Within the geographical limits of the latter, the two are found side by side, and a glance at two panels, one of stone and the other of stucco from Hadda (PLATE V) is sufficient to demonstrate their close affinity. But can we go further, and claim them as facets of one and the same Romano-Buddhist art; or must we differentiate in some more significant sense between them? This question raises the vexed question of chronology in an acute form.

* * * * *

As already indicated, objective evidence of chronology is hard to find. It has been remarked that the earliest certain usage of the Buddha image is no earlier than the 2nd century A.D. Prior to that date, attempts have been made to trace 'Gandhāra' art back to the first centuries B.C.–A.D., to alleged imports into Northwest India from the West and their derivatives under the Parthian régime. If there be in fact an element of truth in this theory, the examples cited in proof of it fail to prove it. They consist mostly of a series of crude works in stone, found at Taxila and ascribed, on grounds which have not been convincingly stated, to the period 30 B.C. to A.D. 40 (20). It was assuredly not from

²⁰ Marshall in *Journ. Roy. As. Soc.* 1947, pp. 6 ff. In no case has any precise objective evidence been cited for this dating; the nearest hint is the statement in respect of one of the figures that 'on stylistic grounds, coupled with the place of finding, this piece may be assigned to the second quarter of the first century A.D.' Of another all that we know is that 'the treatment

[continued on next page]

ANTIQUITY

these uncouth monstrosities that the semi-classical forms of the evolved Gandhāra school emerged. And by the same token we are prevented from attributing the upgrowth of the school to a local resurgence of classical elements handed down from the old Indo-Greek régime of the Northwest; for the decadence of the sub-Greek coinage after the fall of the Bactrian kingdom c. 130 B.C. is eloquent testimony to the lapse of Western artistry in Northwest India and Afghanistan after that date, long before the emergence of Mahāyāna Buddhism. The most that can be urged is that some faint surviving memory of this Graeco-Bactrian art may have prepared the way for the re-introduction of Western classical motifs when the new Buddhism created an appropriate demand.

For the creation of Romano-Buddhist art, then, neither Parthian innovation nor a remote Bactrian tradition can be held primarily responsible, on the evidence at present available. To a possible alternative I shall return later in the present paper.

If, however, the initial date of this art, whether in stone or in stucco, must remain in suspense, the excavations carried out during twenty years by Sir John Marshall at Taxila would appear to provide one important fixed point in the subsequent history at least of the stucco medium. Marshall found uniform evidence that the Buddhist monasteries within the environs of Taxila were 'wantonly and ruthlessly devastated' in the course of the 5th century, and ascribed the circumstance to the invasions of the barbarian White Huns or Ephthalites (21). If this broadly based and very probable view be accepted, we may ascribe the destruction of the Taxila monuments to the years following A.D. 455, the date of the accession of Skandagupta whose Bhitari (Ghāzipur) inscription contains the first reference to a war with the Hūnas. The actual destruction may have occurred then or a little later, in the time of the (probably Hūna) king Mihirakula, who ruled from Sākala (Sialkot) at the beginning of the 6th century and is credited by the celebrated Chinese pilgrim Hiuen Tsang with the demolition of 1,600 stūpas and monasteries and the slaughter of 900,000 lay adherents of Buddhism. When the Chinese ambassador Sung-Yung passed through Gandhāra in 520, he noted that 'this

continued from previous page

of the falling dhoti folds reminds one of the acanthus leaf decoration in the small early stūpa in Block E of Sirkap, dating from the 1st century B.C.'—surely a difficult and tenuous comparison on which to found chronology! Of yet another we are told that 'the early date of this piece is indicated by the sketchy treatment of the drapery and the clumsy unsteady chiselling'. The general impression conveyed is that the *style* of these crude works is the main basis of their dating. But, since the essential problem is just that of the chronology of the style, the argument is circular and gets us no further forward along our road.

More important is the unquestionable fact that these selected sculptures have in any case little or nothing to do with Gandhāra and do not as a group contain even the germ of that art. One is a purely oriental figurine with no hint of the Gandhāra style; another is an oriental type with mechanical drapery of a rudimentary kind which only the most determined theorist could link significantly with the West; others, with the partial exception of a decayed 'Demeter' or Hārītī, have scarcely any sort of stylistic value. To affirm, with Marshall, that these poor relics leave us 'in no doubt that the Gandhāra school had begun to shape in the latter part of the 1st century B.C.' is surely mere wishfulness. Their miserable quality indeed induced Marshall himself to infer that 'we must allow some decades at least after the eclipse of Greek rule in Gandhāra for the practice of Greek art to have been largely forgotten' by the authors of these works; and to add: 'We are thus forced to the conclusion that the School of Gandhāra was not the immediate offspring of Greek art in India, but that it arose during the Śaka period, when Greek art was becoming increasingly decadent'. The logical further step is to admit that most of these 'early' works at Taxila have in fact no significant connexion with Greek art, decadent or otherwise.

²¹ *Guide to Taxila* (3rd ed.), p. 20.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

is the country which the Yethas (Ephthalites) destroyed' (22), and we may therefore give 455-520 as the historical brackets for the destruction. We may fairly assume that the sculptures, found *in situ* amongst the Taxila monasteries were extant about the middle of the 5th century and are unlikely to be of later date.

One of these sculptures, a small stucco Buddha still in position in the Jauliān monastery, is illustrated in PLATE VI B. It represents that art stylistically in its mass-produced decadence. The impassive countenance of the Master is surmounted by hair treated in the most summary manner, the waves being indicated in what may be called the 'ladder mode' (compare plate VI A); that is, by straight lines extending from the sacred knop to the brow with curved cross-grooves like the ratlines on a ship's shrouds. Typologically, this might on general grounds be thought to represent the final stage of devolution from the serried masses of freely-rendered curls which we find in the better examples of the style; but theoretical typology is a dangerous argument, and typological arrangements of Buddhist art such as that, for example, attempted by Masson Oursel (23) are sheer waste of time. The Jauliān figure, however, displays a feature of relevant interest in a more positive sense. As the illustration (PLATE VII A) indicates, beneath the damaged ear and jaw can be seen the ear of an earlier rendering which was subsequently covered by the existing features. Here, therefore, is a stratigraphical, objective sequence, which could, however, be rendered significant only by the further destruction of the final covering. So undesirable a procedure was rendered less necessary by a search which revealed to me a parallel example in Patna (Bihar) Museum (PLATE VII B). This stucco head, found at Sahr-i-Bahlol (Northwest Frontier Province), had similarly been refaced anciently and is sufficiently damaged to show the sequence in detail. Under the typical 'ladder mode' hair of the later phase are the relatively free curls of the original work; and the stylistic succession is thus now established beyond all doubt, the more freely rendered hair being prior to the 5th century A.D., when, if not earlier, it was superseded by the 'ladder mode'.

How long after that century this stucco sculpture continued to be produced in its Indo-Afghan homeland cannot at present be said. If we accept the dating of the violent destruction of the Taxila monasteries to the latter half of the 5th century, we must be chary of applying this datum elsewhere. At the key-site of Hadda in Afghanistan, the excavator was careful to note indications of *gradual* decay: the broken stucco-statuary had bowed its head down as upon a bed, or rather upon a succession of beds formed by the gradually rising debris around the dissolving walls (24). The whole establishment appears to have gone into a gentle decline, not at all suggestive of the ferocity of Mihirakula and his Huns. Nevertheless, here too, if less dramatically, destruction and abandonment occurred between the visit of Fa-hien, who found 1,000 stūpas hereabouts in A.D. 400, and Hiuen Tsang who found desolation in A.D. 630.

The lesson of Hadda and Taxila is that destruction came to the Buddhist monasteries of the region in various ways, sometimes more and sometimes less suddenly. It is obvious that a slowly-decaying site cannot provide us with a fixed terminal point; and that the evidence of Taxila cannot be applied in detail to Hadda, where renovation and innovation, on however reduced a scale, may have proceeded for a century after Taxila had been put to fire and the sword. Nor is this all. There is evidence that the century of hostile Hūṇa domination failed to extinguish Buddhism finally in and about Gandhāra. True,

²² S. Beal, *Buddhist Records of the Western World* (London, 1906), I, xcix.

²³ P. Masson Oursel and others, *Ancient India and Indian Civilization* (London, 1934), pl. vii.

²⁴ J. J. Barthoux, *Les Fouilles de Hadda* (*Mém. de la Délég. Arch. Française en Afghanistan*), III (1930), 64.

ANTIQUITY

at Taxila itself, no indication of a post-Hūṇa revival has been recorded; but at Haḍḍa, in stūpa no. 10, were found Sasanian coins indicating that the structure was erected or at least in use about A.D. 600. Again, the coins found by Sir Alexander Cunningham in a stūpa at Baoti Pind in the Rawalpindi district included 'a gold coin of about A.D. 500 or 600'; whilst Ventura found a coin of Yaśovarman of Kanauj, c. A.D. 725-30, in one of the Manikyāla stūpas (25). It is evident that Buddhism survived or revived sporadically in the northwest during the centuries which intervened between the Hun and the Muslim invasions. In other words, until we know a great deal more than we know at present about the later developments of art and architecture in the region of Gandhāra, we must be chary of generalizing from a single peripheral site of limited duration such as Taxila.

With this proviso, the chronological evidence for Romano-Buddhist art may be summarized as follows. First, this art is the expression of the evolved Buddhism which took shape in the 2nd century A.D. and is associated with the Mahāyāna observance. Secondly, it is focussed on the figure of the Buddha, of which, consistently enough, there is no indubitable example prior to that century. Thirdly, resemblances between individual clay or stucco sculptures and academic Western products of the 2nd century (the Hadrianic Antinous type from Haḍḍa, the young Marcus Aurelius type from Taxila, probably the *tabula Iliaca*, and we may perhaps include the Apollo Belvedere type from the Northwest Frontier (26)) fit into the same picture. Fourthly, the subsequent process of devolution and decay had already reached its ultimate stage by the middle of the 5th century, and a logical process of stylistic deterioration is objectively demonstrated by the palimpsest head from Sahr-i-Bahlol. The clay and stucco sculpture of the Indo-Afghan region would thus appear to fall mainly within the 2nd-5th centuries A.D., without prejudice to some measure of survival after the 5th century. Further afield, along the Central Asian tracks, the art probably lingered on for some centuries afterwards, but precise evidence is lacking (27).

* * * * *

It will have been observed that the stone sculpture to which the term Gandhāra is conventionally restricted has failed to contribute any objective chronological evidence. It occurs at Haḍḍa, near the edge of the schist area, but not in any notable quantity. It appears to have been absent from the latest phase of the mass-produced equipment of the Taxila monasteries, though fragments occasionally occurred amongst their (earlier?) débris. Lack of evidence in general reflects lack of controlled research. Seen in proper perspective, the stone sculpture is but a local phase of Romano-Buddhist sculpture, more restricted in space and probably more restricted in time than the stucco counterpart but essentially integral with it. Attempts to separate the stone and the stucco sculptures chronologically and even culturally are unbased (28).

²⁵ *Arch. Sur. India Reports* II (1871), 141 and 159-60.

²⁶ The date of the original of the Apollo Belvedere, best represented by the Roman marble copy in the Vatican, is unknown, but its smooth academic quality may be supposed at least to have retained its popularity into the academic revival of the Middle Empire, to which the copy probably belongs.

²⁷ See, e.g., Aurel Stein, *Serindia* (Oxford, 1921), I, 485 ff. (Miran); III, 1183 ff. (Ming-oi); *Sand-buried Ruins of Khotan* (London, 1903), pp. 459 ff., etc. Also, J. Strzygowski, 'Die Stuckbildnerei Irans', *Belvedere*, Hegt 9 (Berlin, 1934).

²⁸ See Postscript, p. 17.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

The question now arises, how did these Western elements reach the Indo-Afghan region in or about the 2nd century A.D.? We have rejected the claims of legendary Balkh, Mother of Cities, set at the nodal point where trade and Buddhism from India turned eastwards to China; Balkh where for a century after 250 B.C. Greeks and semi-Greeks maintained some vestige of the Alexandrine tradition; whence that same fading tradition was carried to Northwest India in the form of Indo-Greek dominion which lasted until the first century B.C. We have likewise rejected the Scythians and Parthians in whom this tradition merged; for those folk, however philhellenic, have yielded no evidence of artistic attainment approaching or anticipating the standards of Romano-Buddhist art. Nothing that we know of the Parthians as artists or connoisseurs would lead us to regard them as its parents, or even as its foster-parents. Nor, in the period postulated by Marshall as the formative period of this art (above, p. 9, note 20), can we regard the Parthians as significant carriers of Western *objets-d'art* such as might have stimulated the upgrowth of a local semi-Western school in India or Afghanistan. In the time of the Julii and the Claudii (end of 1st century B.C.—third quarter of 1st century A.D.), the route through the Persian Gulf, to say nothing of routes overland, was rendered uneconomically dangerous by Romano-Parthian antagonism and by insecurity within the Parthian state (29). Parthia does not contribute to the solution of our problem.

If Bactria and Parthia fail to meet our needs, we are left with one main possibility: that of maritime traffic with the classical world primarily via the Red Sea and Alexandria, supplemented in the 2nd century from Palmyra via the Persian Gulf. So far as northern India was concerned, this maritime trade debouched upon the Indus valley and the Gujarat coast. There, in the time of the *Periplus*, the middlemen were Scythians or Sakas, with a metropolis at the head of the Indus delta; but the Scythian kingdom with its trade was absorbed by the Kushāna invaders probably in the time of Kadphises II, about the beginning of the 2nd century A.D.

The Indus-Gujarat trade at this time was of no mean order. The *Periplus* (§ 39) names amongst the imports figured linens, topaz, coral, frankincense, glass vessels, silver and gold plate, and wine; amongst the exports turquoise, lapis lazuli, Seric skins, cotton cloth, silk yarn and indigo. The lapis lazuli must have come from northeastern Afghanistan down the trade-route through the Hindu-Kush, the Peshāwar plain and the Indus valley. The 'Seric skins' presumably followed the same route, as did some of the silk (§ 64), although, as later (30), some silk was already exported from China to the Ganges and the Coromandel and Malābār coasts by a more or less continuous succession of sea-routes. The balance of the evidence is that the bulk of the eastern Asiatic trade

²⁹ For a short time after the settlement of the Armenian question by Nero, relations between Rome and Parthia improved, but the *Periplus of the Erythraean Sea*, compiled probably sometime between A.D. 60 and 110 (for the later dating see J. A. B. Palmer, 'Periplus Maris Erythraei: the Indian evidence as to date', *The Classical Quarterly* xli, 1947, pp. 137 ff.) indicates only a restricted trade in the Persian Gulf. The land routes through Parthia seem never to have been secure for any lengthy period. A pioneer effort such as that of Maes Titianus during the pacific reign of Hadrian shows how little was then known by Western merchants about these routes. On the other hand, after the 1st century trade flowed intermittently along the flanks of the Parthian kingdom and there is evidence for an appreciable traffic between Syria and the East via Charax and the Persian Gulf in the Antonine period (see in particular the epigraphs from the Palmyra agora published by H. Seyrig, 'Antiquités Syriennes', *Syria* 1941, pp. 258 ff.). It is relevant to note in this connexion, with Rostovtzeff, the Palmyrene character of the jewellery reproduced in Gandhāra art—see *Revue des Arts Asiatiques* vii (Paris, 1931-2), 209.

³⁰ F. Hirth, *China and the Roman Orient* (Leipzig and Munich, 1885), pp. 173 ff.

ANTIQUITY

with the Mediterranean lands, whether it started out along the silk-routes of the interior or from Chinese ports, converged upon the western coast of India and avoided transit through the Parthian hinterland. A direct result of this procedure was to give a special importance to the branch-route which led southwards from the Oxus plain along the eastern borders of Parthia through the Hindu-Kush and the Indus valley, into and through the Kushāna kingdom.

Happily, we have something more than a literary record of this trade. In 1937 and 1939, in the midst of the ancient Kapiśā at a distance of some fifty miles north of Kābul, the French Archaeological Mission found a now-famous (though still incompletely published) hoard carefully packed into two rooms, one of which if not both was walled up, in an ancient building at Begrām (31). Whether the hoard was originally assembled at Begrām or was transferred thither from another or other Kushāna sites does not affect the main issue. The hoard, one of the most remarkable ever discovered in Asia, comprised a great quantity of elaborate glassware from the Mediterranean, bronzes including statuettes of Herakles-Serapis (PLATE VIII B) and Harpocrates from the same source, and plaster medallions or *emblemata* (PLATE IX A-B) (32) also of classical origin, a superb collection of ivory carvings from India, and remains of lacquer-work from China. Henri Seyrig and I have independently suggested an Alexandrian origin for some at least of the glass (33), and the Herakles-Serapis, the Harpocrates and the plaster-work are consistent with this attribution, although Syria may also have contributed. In any case, sea-transport may fairly be assumed. In date, the glassware ranges from the end of the 1st to the 3rd century A.D. : and its quality, with that of the associated objects, suggests the likelihood that the collection was a royal one, hidden and forgotten in the chaos of the 3rd century, possibly, as Ghirshman proposes, at the time of the overthrow of the Kushāna empire by Shapur I about the middle of the century.

Here, then, from two small rooms on the Kābul plateau, is a visual monument of active trade between this primary Romano-Buddhist region and the West, particularly Alexandria (34), precisely at the period of the efflorescence of Romano-Buddhist art. And we must remember that this evidence is the product of a relatively small excavation in an almost unexplored countryside ; what other witnesses await us we cannot guess.

³¹ The hoard is now distributed between the Kābul Museum and the Musée Guimet in Paris. For the portion found in 1937, see J. Hackin, *Récherches archéologiques à Bégram* (Mémoires de la Délégation Archéologique Française en Afghanistan, IX, Paris, 1939); and R. Ghirshman, 'Fouilles de Bégram', *Journal Asiatique* CCXXXIV (Paris, 1947) 59 ff., and *Bégram* (Cairo : Institut Français d'Archéologie Orientale, 1946).

³² These medallions were doubtless intended as models for metalwork, e.g. the escutcheons or *emblemata* on the inner base of silver bowls. Such models are characteristic of the metal industry of Ptolemaic and Imperial Egypt (Alexandria, Memphis). See T. Schreiber, *Die Alexandrische Toreutik* (Leipzig, 1894), pp. 470 ff.; A. Adriani, *Le gobelet en argent* (Soc. Roy. d'Arch. d'Alexandrie, 1939), pp. 12, 26, 33; and O. Rubensohn, *Hellenistisches Silbergerät in antiken Gipsabgüssen* (Berlin, 1911), *passim*. Outside Egypt, but within the sphere of Alexandrian influence, similar medallions and moulds occur at Sabratha in Tripolitania (museums at Sabratha and Tripoli)—see pl. IXc.

³³ *Syria* XXII (1941), 262.

³⁴ It is appropriate to recall Rostovtzeff : 'The wares exported from the Roman Empire to the East . . . were chiefly products of Alexandrian industry. The active agents in the exchange of goods between the Roman Empire and India and China were the Alexandrian merchants. Without them the commerce with India would probably not have existed'. *Social and Economic History of the Roman Empire* (Oxford 1926), p. 147; cf. p. 259.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

But the pointers to Alexandria already suggest a line of research which does not appear to have been attempted in this context. For Alexandria was, *par excellence*, the home of stucco sculpture in the West. There in the time of the Ptolemies, in and after the 3rd century B.C., Hellenization inculcated, amongst other things, a taste for marble sculpture in the Greek tradition. Unfortunately, good white native marble is not available in or near the Egyptian delta. On the other hand, along the coast to the west of Alexandria a rich bed of gypsum shines like sunlit snow against the dark ultramarine of the Mediterranean. Here, in unlimited quantity, was a ready-made substitute for marble; and from the Ptolemaic period onwards (35) this marble-substitute, in the form of plaster or stucco, was used freely by the sculptors of Lower Egypt (notably Alexandria) both for statuary and for the metalwork-models such as we have seen at Begrām. Sometimes a statue might aspire to a marble face made up with an otherwise stucco head. Sometimes the sculpture would be wholly of limestone save for a veneer of stucco to simulate marble. Sometimes the whole work would be of stucco (36). Examples can be seen in the museums of Cairo, Alexandria and (at least formerly) Berlin. Be it repeated, Alexandria would appear to have been the main creative centre of this stucco art; thence the technique—and doubtless in some cases the actual moulds—spread westwards to Sabratha in Tripolitania, where there are many examples in the museum and storerooms, and as far as Sousse in Tunisia (37). It occurs in moderation on the Italian mainland (38). And, through the medium of moulds or migrant Western craftsmen or both, it found its way far to the eastwards with the trade which, as we have seen (p. 13), debouched upon the Indian coast at the mouth of the Indus or in Gujarat.

However intermittently, we can follow its further travels fairly clearly. In the lower Indus valley itself, traces of it have been found in the Buddhist monastery which caps the famous prehistoric citadel of Mohenjo-daro (39). Thence there is a dearth of modern exploration until we reach the vicinity of the middle Indus and the fruitful monasteries of much-explored Taxila. At this site also, Roman glass of the 1st century A.D., a small bronze Harpocrates like that from Begrām, a Mediterranean amphora, and gem-intaglios and other objects represent a Western trade which doubtless wholly or mainly followed the same route. Across the Indus towards the northwest, the arterial trade-route from the Indian plains bore the art to the Vale of Peshāwar, across the Khyber

³⁵ Stucco sculpture occurs but rarely in Egypt before the Ptolemaic period, though it is occasionally found as early as the 18th Dynasty.

³⁶ See generally, O. Rubensohn, *op. cit.*; also F. von Bissing in *Arch. Anzeiger* 1901, p. 205; E. Breccia, *La Necropoli di Sciatbi* (*Cat. gén. des antiquités égyptiennes, musée d'Alexandrie*, 1912) I, 156, etc.; II, pls. LXXV, LXXXI; C. C. Edgar, *Cat. gén. des antiquités du musée du Caire, Greek Sculpture*, 1903, pp. 21, 69, 71. Outside the Nile Delta, stucco sculpture occurs sporadically in the Mediterranean area, but Egypt (Alexandria, Memphis) was undoubtedly the centre. The application of stucco ornament to Kertch sarcophagi (C. Watzinger, *Griechische Holz Sarkophage aus der Zeit Alexanders des Grössen*, Leipzig, 1905, p. 53) is not comparable; nor, save in the vaguest way, is the occasional preparation of (plaster?) casts of Greek sculpture perhaps as early as the end of the 4th century B.C., see S. Reinach in *Revue Archéologique*, 3rd S., xli (1902) 5 ff. Stucco was widely used in Islamic Iran and occurs as early as the Sasanian period at Ctesiphon and in Syria (Riefstahl, as cited in note 38); but here again no causative connexion with our Romano-Buddhist art can be postulated from the evidence available.

³⁷ *Arch. Anzeiger*, 1907, p. 168.

³⁸ R. M. Riefstahl in *The Art Bulletin* XIII (Chicago, 1931), pp. 457–8; and a good example in the *insula del Serapide* in the *nuovi scavi* at Ostia.

³⁹ J. Marshall, *Mohenjo-daro and the Indus Civilization* (London, 1931), I, 117.

ANTIQUITY

pass to Haḍḍa (Jalālābād), and through the Hindu Kush to Kunduz on the steppe of Afghan Turkestan, where a buried monastery has yielded to more or less casual digging a number of stucco sculptures (Kābul and Mazar-ī-Sharif museums). At that point, the great trans-Asiatic 'silk route' is reached and, turning its back on exclusive Parthia, the art veered eastwards with Buddhism into Central Asia where, increasingly under Chinese influence, it yet carried something of the Western idiom into remote tracts of Chinese Turkestan, visited only by Aurel Stein and a few other venturesome explorers (FIG. 1).

* * * * *

Tunisia to the Tarim Basin—what a range for the dispersal of the elements of a specialized art and idiom! To illustrate in summary a part of this phenomenon, three stucco heads are reproduced here for comparison (PLATE X). All three heads display the same essential features: the heavy impassive face, the drooping eyelids, the 'ladder mode' hair (or a near approach to it), surmounted by a twisted scarf. Yet one of them, the woman's head, was found at the Roman city of Sabratha in Tripolitania and is now in the site-museum there; the other two are characteristically effeminate Bodhisattvas from the Jauliān monastery at Taxila in the Northwest Frontier Province of Pakistan, and from Haḍḍa in Afghanistan! There can be no doubt as to the essential and significant affinity between the three.

The problem thus begins to assume a revealing shape: the development of stucco sculpture at and from Alexandria in response to local needs and resources; the strong commercial link, vouched for by literature and now by archaeology, between Alexandria and India; the development in northwest India (Pakistan) in the 2nd century A.D. of a Buddhism with new observances and canons which demanded a new imagery such as had already been evolved in the West; and the mobility and adaptability of so easy and expressive a medium as stucco—there we have some at least of the necessary components for a historical solution of the problem of Romano-Buddhist art.

* * * * *

But it would be less than justice to Romano-Buddhist art to end on this note. The foreign elements in that art are obvious, and have both a historic and an æsthetic interest. Equally interesting in both senses, is their adaptation and transmutation by the indigenous artist in conformity with an indigenous ideology and an indigenous artistic tradition. The instances where the actual presence of a Western artist or the use of Western moulds may be suspected are very few. Normally, the hand of the Indian or Afghan adapter is unmistakable, and the later mass-produced works, such as those of the Taxila monasteries and others at Ming-oi and Miran in Central Asia are in full measure the products of native workshops or of mobile schools of Buddhist craftsmen. The apparently increasing formality which characterizes the art has indeed a certain analogy with the increasing formalism of late classical and early Byzantine art in the west. In Asia and Europe alike, the growing spirit of introspection and abstraction was producing a somewhat comparable æsthetic pattern. But that does not imply any close continued liaison between East and West. Men's minds were moving of their own volition in a similar direction, though down separate paths, throughout the civilized Eurasiatic world. The art of Buddhism now began in part to petrify in the terms of lifeless formulae, in part to find a new life in the service of a resurgent Brāhmanism. But that is not our concern in the present paper.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

It remains to add that, so far as we know, the stucco art discussed above was a monopoly of the Buddhist community. There is no clear evidence that it had any secular or non-Buddhist counterpart in Northwest India or Afghanistan.

POSTSCRIPT

For half a century after Dr Leitner's demonstration of Romano-Buddhist art, mainly in the form of stone sculptures from the Peshāwar district (the ancient Gandhāra), the term 'Gandhāra' or 'Graeco-Buddhist' was conventionally applied to it. During the last twenty-five years, however, great quantities of stucco and clay sculpture in a comparable style but with a greater range of expression and skill have, as we have seen, widened the problem aesthetically and geographically; and they have induced certain writers, notably Sir John Marshall, to lend them the status of a separate school. This view has received considerable publicity, and I therefore summarize it as nearly as possible in Marshall's own words (40).

The Gandhāra school, Marshall affirms, was in its infancy in the earlier part of the 1st century A.D. It reached its zenith in the 2nd century under Kanishka and Huvishka; declined somewhat in the reign of Vāsudeva and came to an end, soon after his death, with the eclipse of the Kushāna empire by the Sasanids, c. A.D. 240. The date of the school, therefore, falls into the first two and a half centuries of the Christian era. With the exception of a handful of sadly damaged stucco figures, all the products of this prolific school that have survived are sculptures of stone.

For the next 140 years, according to Sir John, i.e. from A.D. 250 to 390, art seems to have been 'all but non-existent in the northwest'. It was not until the last quarter of the 4th century that the advent of the Kidāra or Little Kushāns from Bactria re-established conditions in which Buddhist art could again flourish. A new school then came into being, which is now generally designated the 'Indo-Afghan' school. (If we must have this, I would interpolate my preference for the more precise term 'Hadda' school, from the name of the site which has produced most work of the highest uniform quality). This later school survived until the third quarter of the 5th century, when the White Huns swept down through the northwest, destroying every Buddhist monument in their path. Like the Gandhāra school before it, this Indo-Afghan or Hadda school was born of the soil of the northwest, and naturally inherited much from its predecessor, the Gandhāra school. But there were radical differences between them. Whereas the earlier sculptors had employed stone as their principal medium, the later employed clay and stucco, and thanks to the plasticity of these materials, they attained a command of form and a vitality of expression which are lacking in the more academic work of the 'older' school.

Sir John goes on to remark the absence from the 'later' school of the *jātakas* and histories which occur in the 'earlier'; and the more limited distribution of the 'earlier' which is 'confined to the Peshāwar Valley and the adjacent country west of the Indus', whilst 'its successor flourished over a much larger area, including part of the Punjab east of the Indus'.

I have been at pains to reproduce at some length the views of the excavator of Taxila because they represent at the same time the most precise and presumably the most widely founded classification of Romano-Buddhist art yet attempted. If in important respects I find it unacceptable, I must repeat the warning that the details of the Taxila evidence remain unpublished. I find it difficult to believe, however, that Sir John has up his sleeve sufficient objective information to support a view which only overwhelming evidence of a most detailed and exacting kind could substantiate.

Let us for a moment consider the generalities of that view. First, it crowds quite impossibly into little more than half a century (c. A.D. 390-455) an immense output of clay and stucco sculpture of very widely varying character and quality (compare our pls. IIB and VIA). Secondly, for this stucco renaissance it chooses a date of peculiar difficulty. 'It was not until the last quarter of the 4th century', declares Marshall, 'that the advent of the Kidāra-Kushāns from Bactria put an end to Sasanid domination in the northwest, and, by restoring the political and cultural unity of the

⁴⁰ *Journ. Roy. As. Soc.* 1946, p. 119; *ib.*, 1947, pp. 16 ff.; also *Guide to Taxila* (3rd ed.), pp. 32 ff.

ANTIQUITY

country east and west of the Indus over which they ruled, re-established conditions in which Buddhist art could again flourish. A new school then came into being, which is now generally designated the Indo-Afghan 'school', etc. But this reading of the admittedly obscure history of the period would be hard to substantiate. It would appear that in the middle of the 4th century the Kidāra or Little Kushāns ruled from Peshāwar as feudatories of the Sasanian king, whose writ ran as far eastward as the Indus valley. Twice, perhaps c. 355 and 368, the Kidāras threw off the Sasanian yoke but on both occasions they were brought back to a state of restless dependency. Attempts at aggrandisement towards the east seem to have secured no more lasting gain from the Gupta empire (41). Indeed, between the scissor-blades of the Sasanians and the Guptas, the ambitions of the Kidāras were foredoomed to frustration. One may search in vain in their history for any context for an artistic resurgence of the highly sophisticated and exotic type implied in the 'Indo-Afghan' school of sculpture at its prime.

Thirdly, Marshall assumes between stone and stucco a hiatus of 140 years after which, from no known source and under no known stimulus, the Western idiom suddenly reappears in at least as emphatic a form as in the 'Gandhāra' phase preceding the hiatus. And in any case, after notorious warnings in other ages and places, it is a bold man who will to-day postulate any such hiatus in the development of a culture which is otherwise integrated by so marked a community of inspiration.

Fourthly, there is no authority whatsoever for the *ex cathedra* statement that Gandhāra stone sculpture ended c. A.D. 240 or 250. Fifthly, the Taxila evidence cited (up to date) for the emergence of Gandhāra art in the first centuries B.C.-A.D. is entirely unconvincing. Sixthly the geographical factor is quite inadequately stated and considered.

On all these grounds I find Marshall's classification and chronology unacceptable.

⁴¹ For a recent summary of the history of the period, see R. C. Majumdar and A. S. Altekar, *A New History of the Indian People* vi (Lahore, 1946), 21 ff.

ROMANO-BUDDHIST ART: AN OLD PROBLEM RESTATED

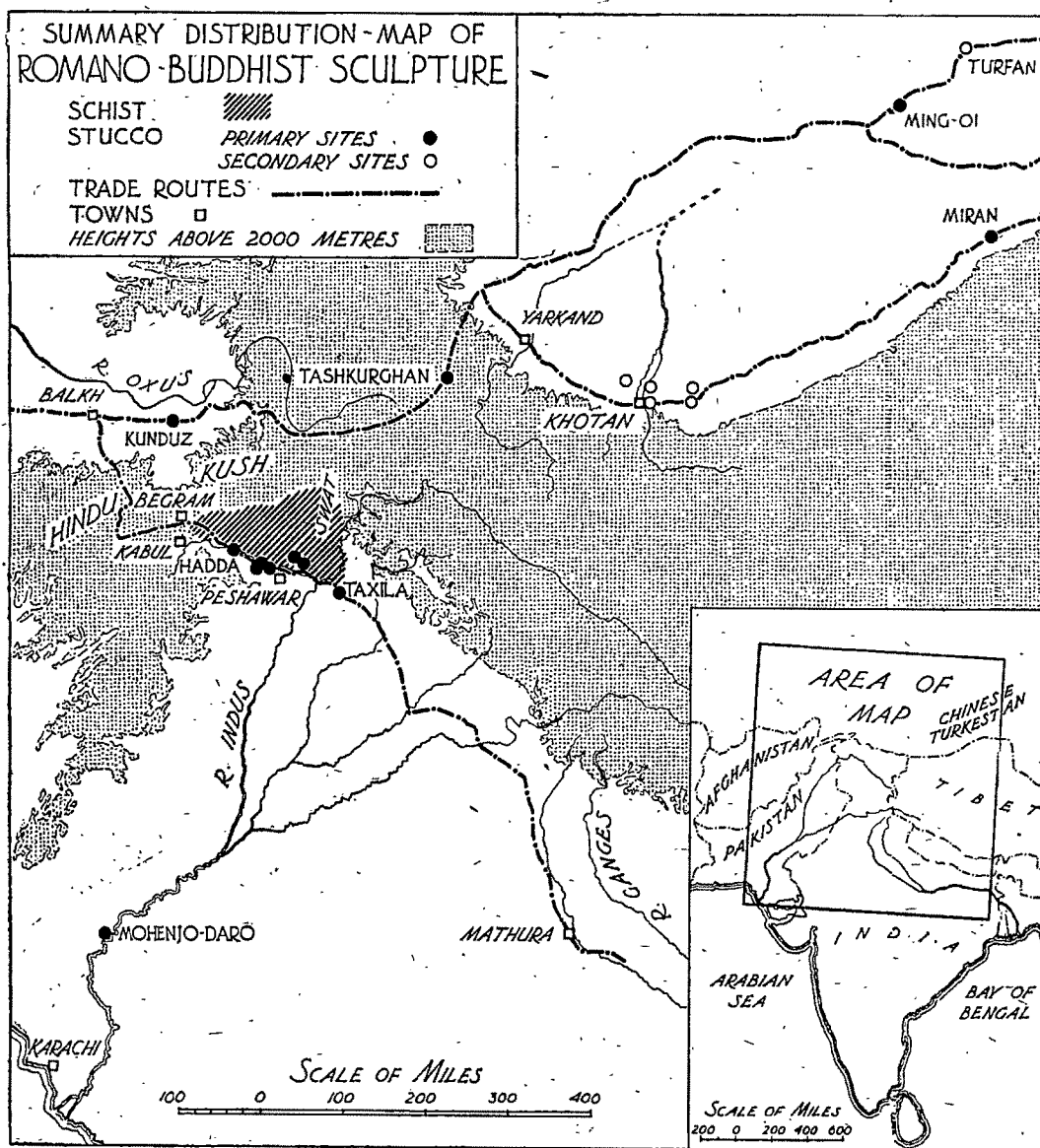


FIG. 1

The Dumb and the Stammerers in Early Irish History

by HUBERT BUTLER

PRIMITIVE peoples judge other races strictly by the yardstick of their own virtues and capacities. Those, who do not conform to their customs, are mad or stupid. Those, who cannot communicate with them, are dumb or have an impediment in their speech. If they are obliged to admit that the foreigner with all his defects can do certain things remarkably well, they are more likely to ascribe this to magic than to superior but distinctive capacities. When the newcomers are, like the Spaniards in the New World, the Europeans in the Pacific, so dazzlingly accomplished that it is hard to disparage them, the natives welcome them as supernatural beings more readily than as a more developed race of men. They will change their gods more willingly than their good opinion of themselves as the norm of humanity. The Greek word for people whom they could not understand was *βάρβαροι* or 'stammerers', the Russian word for the Teutons, with whom communication was difficult, was 'nyemets' or 'dumb'. A variant of this word was used by all the other Slavonic peoples. Many less familiar instances of this practice could be collected. These names were retained for centuries after the idea that mankind was divided into many races equally but differently equipped had become familiar.

Ireland has seen many invasions. To take the most recent and widely discussed classification of them, T. F. O'Rahilly suggests that the invaders, after prehistoric times, came successively in the following waves. First Cruthin, second Erainn, third Lagen, fourth Goidels. Of these the first three were P-Celts, the last only, Q-Celts. It would be natural, therefore, to look for evidence of difficulty of communication between the several population groups. Yet the traces have been obscured not merely by time but by a deliberate policy. The chroniclers and genealogists of the Irish people made many elaborate attempts to efface the memory of racial friction. When the Goidels, who, if we follow O'Rahilly, were the last comers, had asserted their supremacy over the other races in Ireland, their *literati* furnished all these others with Goidelic pedigrees. In this way the Goidels, besides conciliating their neighbours, who were often also their maternal kinsmen, were able to persuade themselves that they had lived in Ireland from the most remote periods and were in no sense *parvenu* conquerors. Legends and stories, in which differences of language were implied, would be given a twist that made them meaningless or suggestive of something different. Often our attention is arrested by stories of people, who were wrongly called 'dumb' or who were cured of dumbness by a miracle. Probably not only the Goidels but also the Lagen and their other predecessors covered their traces in this way, so that it is not easy to say by whom the taunt of dumbness was flung at any particular tribe or who later explained it away in the interests of harmony. All we can do is to collect these legends of language-trouble on a loose strand of conjecture and leave it to the philologist and the folk-lore student to order them correctly.

The greater part of the legends which I shall examine refer to the southeast of Ireland and particularly to Ossory, a region, which corresponds roughly to the modern county of Kilkenny, enlarged to the north by several baronies in Leix and Offaly; but

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY

O'Rahilly, seeking for traces of the language of the Belgic Erainn, which he equates with 'Iarnbéire', (1) relates a legend from another quarter of Ireland, which is worth recalling, because it follows a pattern, which is many times reproduced in many regions. There are two different versions of this story of the first Goidelic landing in southwest Ireland. In the earliest (2), Eogan, when he landed, found the natives speaking a different speech or dialect to his own. In the later (3) version this tradition is purged of the disagreeable implication that he was a foreigner. Eogan does not invade. He is an Irishman returning to Ireland. It is not stated that the Munster chieftains, Conaire and Maicnia, whose forces were ranged against him at Carnbuidhe near Kenmare, speak a different language. It is merely said that the first messenger gets ignored, the second one at last makes himself understood by Maicnia. It is not necessary to say that the Munstermen were dumb, they were merely preoccupied or obtuse.

The legend of Maon or Labraid Loingsech's invasion, though it refers to an earlier period, was probably shaped under the same influences and along similar lines. O'Rahilly believes it to be the record of the Lagenian invasion (4) and Labraid, who is only imperfectly distinguishable from his grandfather, Laoghaire Lorc, he supposes the ancestor deity of the Lagen euhemerised by the historians into a warrior-king. Labraid means 'the speaker'. Maon means 'dumb'. Lorc is glossed 'balb', i.e. 'dumb', in the Lecan and Stowe glossaries.

Ridgeway, before O'Rahilly, marked the importance of this story. Here is a band of invaders from Gaul landing in Wexford and bearing a new weapon, the 'láigen' or spear from which Leinster was, perhaps fancifully, said to derive its name (5). The historians, harmonizing, make Maon related dynastically to Cobhthach, the king of the Erainn of South Leinster, whom he overthrows. He was not, it is implied, any more than Tuathal Techtmar or Eogan, an invader; he was a native returning to throw out those who had usurped his throne. Did his race bring a new speech or dialect together with a new weapon? I think it arguable that they did and that his various contradictory names are evidence of a certain embarrassment about acknowledging this fact. The legends betray a similar confusion of purpose.

For example (6), a druid, who was in the fortress of Dionn Riogh near Leighlinbridge, where Cobhthach was slaughtered, enquired who was the murderer. 'The Seafarer', replied a man outside. 'Does the Seafarer speak?' enquired the druid. 'He speaks', ('Labraid'), replied the other. Hence the name, Labraid Loingsech, clung to Maon ever since. This anecdote is quite pointless except to refute the implications of Labraid's other name, 'Maon' or 'dumb', with its suggestion that he and his people were foreign invaders (7). The Erainn, according to the theory, were P-Celts, like the Lagen, who

¹ T. F. O'Rahilly, *Early Irish History and Mythology*. p. 89.

² Laud, 610 (printed in *Zeitschrift für Celtische Philologie*, VIII, 312 f.).

³ Cath Maige Lena, O'Curry 1855.

⁴ O'R., op. cit., p. 106.

⁵ Ridgeway, *Early Age of Greece* II, p. 545.

⁶ Keating, Vol. II, 165.

⁷ This interpretation of Labraid's name need not exclude other meanings. In an earlier, less anthropomorphic stage of his development, he may well have been the Thunder-God, whose speech was thunder. He would be comparable to Meldos, the Thunder-bolt God, whom O'Rahilly believes to have been the tribal deity of the Meldi. op. cit. 53. 'In the Sick-bed of Cu-Chulainn' (Cross and Slover, *Ancient Irish Tales*, p. 183) there is a fairy-king, Labraid of Mag Mell (O'R. derives Mell too from the root-word 'thunder-bolt'), whose chariot was heard rattling across the waters, portending to those that listened that his spirit was gloomy.

ANTIQUITY

conquered them, and the legend might conceal some admission of kinship, a recognition of a common basis to their languages. It is as likely though to have been a Goidelic as a Lagenian anecdote, with the purpose of denying that the inhabitants of southeast Ireland, whatever their provenance, spoke a distinctive non-Goidelic tongue. If such a tongue was, in fact, spoken, when the legend was promulgated, it would be discredited as a mere local patois, for, according to the legend, the ancestor gods of the region would be shown to speak Goidelic.

The southeast of Ireland, where the Lagen landed, managed for many centuries to preserve an independent character, passing alternately under the influence of the Goidels of Munster and of the Midlands. When the lives of the saints were being written, there probably still survived memories of idiosyncrasies in the habits, beliefs and speech of the Ossorians and their neighbours. The efforts of the hagiographers to belittle these differences are not always successful. They seem to follow the same practice as historians, guarding against any inference that their heroes, the saints, were newcomers, bringing new ideas, or that they ever in any way associated themselves with foreign invaders. Christianity appeared first in Ossory from the west, at the time when Aengus MacNadfraech, the Goidelic king of Munster, and his clients, the Corcu-Loigde and the Dessi, were extending their power westwards. O'Rahilly thinks that these two tribes, as well as the Ossorians, were Erainn (8), and there is something to be said for the view that they were both regarded as peculiar and primitive in their culture by the Goidels. The Ossorians seem to have been alternately dominated by the Lagenian Clann Connla and the Erainn Corcu-Loigde.

St. Kieran was the first bishop and saint of the Ossorians, though it was a later saint, Canice, who gave his name to the town and county of Kilkenny. Kieran's associations are chiefly with Munster, Canice's with the midlands and the north of Ireland but it is in the very north of Ossory, under the Slieve Bloom mountains, that tradition places their monasteries. These mountains remained for centuries a region of pre-Goidelic peoples, Cruthin, Erainn and Lagen. The two saints also have strong associations with Scotland and Wales, and there are dedications to Kieran in Cornwall and Brittany. Canice, in particular, seems to have had some special affinity with the Cruthins and Picts of Ireland and Scotland. They would both, according to O'Rahilly's theory, belong to the P-Celtic peoples, at any rate by adoption. There is a powerful tradition that St. Kieran preceded Patrick in evangelizing the Ossorians and there are many stories of disharmony between the two saints and their followers, which the biographers of Patrick and Kieran have not completely succeeded in discrediting.

St. Kieran's mother was one of the Corcu-Loigde from Cape Clear in West Cork and he is said to have been born there (9). Yet since the Corcu-Loigde dominated Ossory for a time and were then thrown out by the Clann Connla an effort was made to show that St. Kieran, the patron saint of Ossory, was a real native Ossorian and not a foreigner. That is the only way in which I can explain the queer reference to him in the *Felire of Aengus*.

Associating him with his successor as abbot of his monastery of Seir-Kieran, Carthach, the stanza runs :

⁸ O'Rahilly, *op. cit.* 18.

⁹ Plummer, *Vitae Sanctorum Hiberniae*, Vol. 1, 217. But O'Rahilly in *The Two Patricks*, suggests that Kieran and the other pre-Patrician saints were British. Even so, his cult would have come to Ossory through the Corcu Loigde.

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY

' Bounded the Stutterer,
His fame across the salt (Sea) eastwards.
The royal Carthach, the Romish,
The multitudinous Kieran of Saighir '. (i.e. Seir-Kieran).

But over the first line the scholiast has made a gloss with reference to the stutterer, which has been translated ;

' or not-silent
i.e. not-stuttering or undistinguished ' (10).

(John Hogan, Kieran's 19th century biographer improves on the gloss by saying that 'not-stuttering' means 'eloquent'!)

If the translation is correct and 'not-stuttering' was equated by the scholiast with 'not-undistinguished', we have advanced a further stage towards proving that the undistinguished subject races, whose language was unintelligible, were called 'stutterers' or simply 'dumb'. By birth-place and blood, Kieran was closely associated with the P-Celtic tribe of Corcu-Loigde, as we have seen, and this in course of time became a discreditable association which had to be vigorously denied. Hence both the stanza and the scholiast's defiant gloss upon it. According to the same principle, 'Maon', the 'dumb', had to be rechristened 'Labraid', 'the speaker' (11). The Felire is dated some time before 778 and it would appear that when it was written it was still possible to admit that there had been eminent Irishmen, who were not Goidels (12).

The Dessi, too, appear in Ossory history as a distinctive and ill-comprehended people. The cannibal practices of their fosterling, Ethne the Horrible (13), and her devotion to her mother's people rather than to her father's or her husband's, the stories about these things are such as you would relate, true or false, of a tribe with which you shared no tradition of speech or origin. It is likely that they were an earlier people than the Ossorians, but, as they had obvious matriarchal habits, they may easily have acquired an alien aristocracy. The story of their wanderings from Meath to Ard-ladran in south Leinster and thence to Milledach in south Ossory and finally to Munster may in that case be connected with the marriages of their princesses. Their Goidelic consorts, Crimthann of South Leinster, the husband of three Dessi princesses, and Aengus MacNadfraech of Munster, who married Crimthann's daughter, Ethne, may have chosen to obtain new spheres of influence and legitimize their oppressions by marrying the

¹⁰ Translation by Hennessy, quoted by J. Hogan (*St. Ciaran of Ossory*, p. 69). Whitley Stokes in a slightly different translation interprets the crucial phrase 'unsilently', but 'balbdai' means literally and primarily 'in a stuttering manner'. The translation Romish for ruamach is not now acceptable.

Carthach was the son of Aengus and Ethne the Horrible. He had a brother called Faelan Balbh, who is identical with St. Foelan Amlabhair. (See note 31).

¹¹ See Ovid, *Fasti* II, 583, for a similar antithesis, which might admit of a similar explanation. Muta, the mother of the Lares or household gods, whom the Romans adopted from the Etruscans, was a nymph, who talked too much and was struck dumb by Jupiter. The Lares, on the other hand, derived their name, according to Latin philologists from λαλᾶν to speak.

¹² Kenney, *Sources for the Early History of Ireland*, p. 481, dates bulk of glosses to Felire to 11th century or later.

¹³ *Y Cymmrodor*, 12-14, p. 119. 'On account of this maiden', says the Druid, 'her mother's kindred shall seize the land on which they dwell. So they reared her on the flesh of little boys . . . little boys dreaded her . . . so that she would grow quickly'.

ANTIQUITY

female leaders of the Dessi (14). The Dessi were a widely scattered people, and, when we are told of their migrations, the intention may be to account for their tribe being known in several different parts of Ireland as well as Wales and their queens, who, no doubt, had a spiritual sway over them, associated, in turn, with different peoples. Thus the Dessi, a branch of a once widespread primitive race, would have been indigenous in all the regions to which or from which they are supposed to have migrated. Their departure from any district would merely mean that they had accepted alien rule and only by their customs or their cult, that of Declan for example, would they be distinguishable from their new rulers. In this way I would explain the alleged evacuation of south Ossory by the Dessi and also the persistent tradition that a strange and primitive people continued to live there. As regards the Dessi marriages, I do not think the case would be altered, if it was held that the Dessi queens were ancestress goddesses. Supernatural alliances follow in the tracks of natural ones. Thus according to Ridgeway (15), the marriages of Menelaus and Helen, Xuthus and Creusa, Pelops and Hippodamia, etc., betoken the fusion of the Achaean invader with an older and indigenous people, who practised matriarchy.

East and northeast of the Ossorians lay the Lagen of Wexford and Carlow. To the north there were the Cruthin of Leix and the Eli to the northwest, who, O'Rahilly (16) thinks, were more likely to be Lagen than Munster Goidels. Westwards and southwards lay the Goidels of Cashel and their allies the Dessi.

Who were the Ossorians themselves? They claimed to be Lagen, 'but', says O'Rahilly (17), 'their very insistence on this point reveals that they were propagating a novel theory'. In fact it is likely that they were a people of mixed Lagen and pre-Lagen descent. It is well known that for a century and a half a large part of Ossory, including the northern province of Ui Duach, was under the sway of Corcu Loigde kings (Erainn), brought there, it is said, by Aengus MacNadfraech, the Southern Goidel. It is possible that this rule was written up as a usurpation, like that of Cobhthach at Dionn Riogh or Cairbre Cathad at Tara, only after it had come to an end, that, in fact, they were indigenous in north-Ossory, as the Dessi were in the south, but did not become formidable to the Lagen, till they passed like their kinsmen, the Corcu Loigde of West Cork, under the patronage of the Goidelic kings. When later, they came under Lagen control, naturally their gods, Laeghaire Bern Buadach, for example, would be inserted into the Lagen pedigree.

It appears that from very early times south Ossory lay on a different invasion route to the north; the megalithic belt, a very rich one, hardly reaches beyond the Walsh mountains, the Ogham belt does not extend north of Clara near Kilkenny city. The range of sculptured crosses is similar. I think it would be true to say that southern Ossory preserved always a more intimate relationship with Munster and the west than did north Ossory. After the eclipse of the Corcu Loigde, north Ossory was orientated towards the Lagen and the Midland Goidels. Earlier, Eremon, who had made his capital at Rathbeath in Ui Duach, had been the leader of the Midland, not of the Southern Goidels. Though later on the Southern Goidels, under Aengus MacNadfraech, may have made inroads, their influence was not a persistent one.

One can only guess at the racial origins of the Ossorians, but I think it is plain that marked differences of dialect and language existed among them. The lives of the saints were written at a time, when the Goidelic tongue had supplanted others as a literary language. With immense arrogance the scribes had decided that what the others spoke

¹⁴ The King of Ossory, Conchraid, also seems to have been on the verge of an entanglement with a Dessi princess, but he was saved from it by St. Ciaran. Plummer, *V.S.H.*, I, 224.

¹⁵ Ridgeway, *op. cit.*, Vol. II, 101.

¹⁶ O'Rahilly, *op. cit.*, p. 21.

¹⁷ *op. cit.*, p. 18.

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY

was not speech at all. Yet at times they were obliged to eulogize saints, whose associations with these speechless zones was too well-attested to be ignored. Ossory, and particularly southern Ossory seems to have been a speechless zone.

I have said that the Dessi were traditionally supposed to have settled at Milledach in south Ossory (by the confluence of the Barrow, Nore and Suir). They were quickly evicted, it is said, by the Ossorians (18). In revenge they plotted with Aengus and his queen Ethne, whom they had fostered and who considered herself a Dessi princess like her mother. Together they won the district of Femen from the Ossorians. Yet it is not recorded that, in their moment of triumph, they recovered their former land at the confluence. I suspect that they never lost it or left it but that what its inhabitants lost or were discouraged from acknowledging was their sense of kinship with the Dessi of Waterford. They became closely associated with the southern Ossorians, their neighbours, who treated their queerness not as a distinctive national culture but as a nasty divagation from the Ossorian norm.

There are three curious anecdotes, which, taken together, have a bearing on this. The first one comes from the Life of St. Declan (19). We read there how Declan, when he was walking one day through south Ossory, asked for a night's lodging in a certain settlement. He was insulted and forcibly repelled. To avenge him the Lord killed sixty of the inhabitants and only two men and ten women, who had opposed the ill-treatment of Declan, survived. These offered him the place for a monastery and he made Kilcolumderg there (the name still survives), and put one of the Dál Mess Corb, called Columbus Rufus, in charge of it, withdrawing himself to Dessi territory.

The next anecdote is told about a cairn in the same parish of Kilcolumb (20). St. Patrick had come to the hill, where the cairn later stood. He was charmed by the site and started to build a city there. While they were working away at the foundations of the cathedral, a pagan woman came up with a dish of roast meat. Patrick did not like the look of it. He thought he saw a dog's paw in the gravy. He made the Sign of the Cross over it and prayed. The meat leapt out of the dish in the form of a yellow dog and ran towards Waterford. St. Patrick called the spot 'Cunniawee' or 'Yellow Dog' and pronounced an awful curse on the woman, her descendants and her neighbourhood. The curse includes the lines:

'Accursed the black breed of this woman',
Who served to me this filthy hound!
From their wry mouths henceforth no man
Shall hear but foul, impious sound!

Of the place itself he declared:

May all good men for e'er dislike it,
May it be cursed with deaf and dumb!

The third anecdote is very similar to this but it refers to St. Declan and to the district of Femen, round Slievenaman, which the Dessi took from the Ossorians (21). Declan was wandering there one day, when he was invited to a meal by a sly, rich pagan. Again the principal dish was a fat roast dog and Declan caught sight of the paw in the gravy. The trick was exposed, the land forfeited in penance, and a monastery built called Coninga or 'Dog's Paw'.

¹⁸ For the story see *Y Cymynrodor*, 12-14, p. 119.

¹⁹ *V.S.H.*, II, 51.

²⁰ John O'Donovan, Ordnance Survey Letter on Kilkenny, quoted in Carrigan *History of Ossory*, IV, 98.

²¹ *V.S.H.*, II, 50.

ANTIQUITY

What can we deduce from these curiously overlapping and yet conflicting stories? What had the narrators in view? In the first place they had to account for the survival in the Kilcolumb district of the cult of the Dessi saint and of another saint unfamiliar to Ossory. Columbus Rufus, who succeeded Declan at Kilcolumb, was of the Dál Mess Corb, a powerful tribe originating in the Arklow region (22). It would be natural enough that in this border territory, the cults of the Dál Mess Corb, an eastern tribe from Wexford and Wicklow and of the Dessi, a western tribe, should meet. That the meeting was not a friendly one is suggested by the sixty deaths. It is a principle of hagiology that though tribes may disagree, tribal saints always make up their quarrels, so Columbus Rufus appears in the life of Declan as the disciple, not the supplanter of Declan. The Dál Mess Corb, like the Dessi, were certainly a pre-Laginian tribe (23), and plainly there was some primitive, much disliked cult of dogs in these border territories of Femen and Ida, with which the Dessi and other tribes of the Erainn were associated. (There are dolmens called 'Graves of the Hounds' at Garryduff and Kilmogue in this district of southern Ossory, which no doubt came to be connected wrongly with the dog-folk) (24).

In the Life of Declan it is Declan himself who is served with roast dog by the pagan; in the later legend of Kilcolumb in Ida the association of the neighbourhood with the Dessi has been forgotten or ignored and it is Patrick, who gets the bad reception. It is remarkable that in several other parts of Ossory there are legends of St. Patrick receiving a poor welcome (25). This means, I think, that it was some time before Patrician Christianity got a foothold here. The stories commemorate some not completely successful struggle between Christianity and Paganism or heresy, which has been confused with an earlier struggle of Goidels or Lagin against a primitive people, who were 'deaf and dumb' and from whose 'wry mouths' there issued nothing but 'foul impious sounds'.

The story of the imprisonment of Scanlan, king of Ossory, by the High King and St. Columba's intercession for him, and of Scanlan's miraculous escape is fairly well-known (26). I shall only repeat one passage, because it illustrates once more the widespread opinion that the Ossorians were a distinctive tribe. When the angel released Scanlan, the prince made his way to the church near Derry where Columba was at matins. When Columba asked for his news, he replied panting, 'Deoch! Deoch!' (A drink! A drink!), so great was his thirst. Because he could say nothing but, 'Deoch!' St. Columba left an impediment in speech to his posterity (or, as another version has it, to the kings of Ossory, who came after him) (27). Scanlan appealed against this curse,

²² Other saints of the Dál Mess Corb were Abban and Dagan and the Kevin group of saints, who were associated with Wicklow and Wexford.

²³ O'Rahilly, *op. cit.* 27.

²⁴ Carrigan, *op. cit.*, IV, 248-9. For other dog and dolmen stories, see O. G. S. Crawford, *Long Barrows of the Cotswolds*. The same association occurs in Wales.

²⁵ For hostility of Uí Duach of Ossory to St. Patrick see *V.S.H.* I, 222, also *Kilkenny Archaeological Journal*, 1876, 191-3, and John O'Donovan, *K.A.J.*, 1849-51, 365. For hostility in South Ossory see Joyce's *Place-names*, Vol. II, 34.

²⁶ For the several versions of this story see Father Shearman, *K.A.J.*, 1876-8, 354.

²⁷ Father John Ryan, S. J. (*Journal of the Royal Society of Antiquaries*, April, 1946) thinks that the Clann Connla, the rival dynasty of Ossory, returned permanently to power soon after Scanlan's death, but this does not seem very probable from this anecdote, which implies that Scanlan had successors of his own blood. It is, of course, possible that the story is not told of Scanlan Mor but of another Scanlan of the Clann Connla; in that case, the dynasty of stammerers would be Lagin, not Corcu Loigde. From the standpoint of the Goidels, both dynasties, being P-Celtic, would be equally difficult to follow.

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY

but Columba told him that it was God's decree, yet to console him he said that there should be many princes and rulers of Scanlan's seed. There was a tradition in County Kilkenny that the descendants of Scanlan did, in fact, stammer. I doubt too whether the narrator would have told this prophecy, if it had been unfulfilled. The story bears witness to the fact that in later times there survived among the Ossorians either a distinctive language or the tradition of one, which the midland Goidels found difficulty in understanding. The story that St. Columba put the curse of stammering on Scanlan's posterity for ever afterwards simply because he asked for a drink too often is otherwise a baffling one, for Scanlan was a protégé of St. Columba. I cannot resist the suspicion that the word 'deoch' conceals some pun or provincial barbarism, which a philologist might unravel (28).

It would be premature, though tempting, to build a theory of racial origins on facts such as these. The most one can do is to draw attention to their oddity and to hope that its significance will one day be better understood. There are other anomalies, which cannot be overlooked. Why, for example, was Banba, the first woman, who took Ireland before the flood also called 'Balba' (29). Was Mac Da Tho really the son of two mutes? (30) How could St. Foelan Amlabhair be the first Christian missionary to Scotland and yet be dumb? (31) Who was the stammering king of Ireland, Suibhne Menn? (32). Who was the Scál Balbh (33), and why was his name attached to the Leac an Sceal Bhailbh, the largest of the dolmens in south Kilkenny?

The Ulaid (the Ulstermen of the Saga), were once P-Celts, in O'Rahilly's view, so it is natural to ask, whether Cuscraid, the Stammerer, Conchobar's son, really owed his nick-name to the fact that a spear went through his wind-pipe (34). And was it really only because they loved him and wished to flatter him that all the women of Ulster started to stammer too (35).

Then there was an Ulster saint, Darerca, who acquired her other name of Moninne from a dumb poet. The *Felire of Aengus* gives her day as July 6. 'Moninne of Sliab Cuillinn, who was previously named Darerca, or Saibile was her name previously. But a certain dumb poet fasted at her that his speech might come to him. And this was the first thing he said, i.e. Nin Nin. Hence the nun was called Mo-ninne and the poet Ninine eces'. Surely, though, what happened was that the saints of two tribes, one

²⁸ The anecdote has a family resemblance to that of St. Moninne's second name (see below).

²⁹ She and Ladran and his fifteen other female companions curiously took the same route as the Dessi, going from Ard-ladran to Milledach. Ultimately Ladran died of 'female persecution'. Lebor Gabala, *Irish Texts*, II, 187, 191, 207.

³⁰ O'Rahilly, op. cit., 485, derives the name from Da To, the dumb god.

³¹ Watson's *Celtic Place-names of Scotland*.

³² *V.S.H.* II, 369, cf. also Colla Menn, ancestor of the Mugdorni, a primitive tribe, one of whose septs was Papraige, therefore, supposedly, P-Celtic. Cf. also Moen MacEtna, a poet. O'R. op. cit. 103.

³³ He was also Cian, the father of Lug. O'R. op. cit. 61, 103.

³⁴ The attribute 'stammering' may, of course, sometimes have derived from the intercourse of the Irish with foreign missionaries or other visitors, Romanized Celts perhaps from abroad. It is a double-edged word which can be used equally well by either party to the misunderstanding. In Irish monastic circles on the continent, the names Balbus and Balbulus seem once or twice at least to have been used as pseudonyms for foreigners. Kenney, op. cit. 568 and 596.

³⁵ 'Story of Mac Da Tho's Pig, trans. by Kuno Meyer.

ANTIQUITY

dumb, one speaking, had come to be identified (36). Moninne was of the Cruthin or Irish Picts.

As for the 'intolerable language of the bards' (37), which is constantly mentioned, does this refer to cursings only? Hardly. A language that is obsolete or foreign often possesses peculiar magical or sacramental qualities and survives in blessings and solemn denunciations long after it has died out in ordinary speech (38). St. Columba, the defender of the bards, was also the patron, like St. Canice, of the Picts and of the 'stammering' kings of Ossory. When at Seir Kieran the Crossans or local bards chanted a dirge over the grave of Donnchad, king of Ossory, why were the clergy and better class mourners and the local Culdee so revolted that they started to vomit? Why did they consider them demons? Why did they have to fast and to sprinkle holy water on the grave? The words of the dirge, which Keating records, are highly respectable, eulogistic, Christian. He relates that two local Crossans, who were at the funeral, and do not seem like demons, committed the dirge to memory. Does not that mean that the words were unintelligible gibberish to the bystanders, till the bards translated them and therefore the language of the dirge was as ill-omened as the peculiar mourning garb of the Crossans? Is it not possible that in that wild district of the Slievebloom, with its many minor tribes, the Crossans retained a pre-Goidelic language and Christian cult, even in the tenth century, a cult and language, which the Ossorians themselves had long shed and which survived for them only in blasphemy and forbidden ritual? (39).

There may be a straightforward answer to many of these questions but, if they are taken in conjunction, the obvious explanations fail to satisfy.

In regard to language difficulties, I do not think that the many queer stories about books have been sufficiently examined. Many books were clearly of magical importance because of their antiquity and long association with a people or neighbourhood. At least one lot of books, as we shall see, was so old and unintelligible as to be almost sinister. Often the deeply cherished books, in troubled times, must have disappeared, but they were usually, as we read in the lives of the saints, restored as good as new. The holy books, which saints leave out in the rain or drop into ponds or lose in conflagrations, always survive these catastrophes unimpaired. In this way, when a community loses the venerable treasure which attracts pilgrims from afar off, a new book soon inherits the veneration and authority of the old. Yet I think there must have been a tradition among the simple folk always that the oldest books, even when their orthodoxy or refinement was open to question, had the most magical properties.

This would account for the strange mixture of repining and complacency, with which the loss of Lon Garad's library is related (40). This saint, who was also called Cois Finn or White-Leg, lived at Disert in north Ossory. He was a very learned man and, on his account, St. Columba made another appearance in Ossory legend to inspect his books. Columba, as we have seen, was a patron of linguistic oddities. Lon Garad refused to

³⁶ 'Sickbed of Cu-chulain', Cross and Slover, *Ancient Irish Tales*, p. 178.

³⁷ See Kenney, *op. cit.* 427.

³⁸ For divine words as an archaic survival, perhaps from an older race, see Leaf on Iliad. I. 403 and XX, 74. *ὁν Ξάνθον καλέουσι θεοί, ἄνδρες δὲ Σκάμανδρον.*

³⁹ Keating, III, 216-221. Charles Autran, 'Homère et les origines sacerdotales de l'épopée grecque', p. 76, gives many instances of this, e.g. the Gaelic of Cornwall, replaced by Brythonic; survives in 'The Fairy Speech'. In Calabria till 1910 girls sang to the goddess in Albanian, an ancient local idiom, on July 27, the day of Santa Venere.

⁴⁰ Felire of Aengus, Sep. 3. Martyrology of Donegal, Sep. 3.

PLATE I



b. TERRACOTTA HEAD FROM THE NORTHWEST FRONTIER, PAKISTAN
(Eric Dickinson Collection). About half life-size

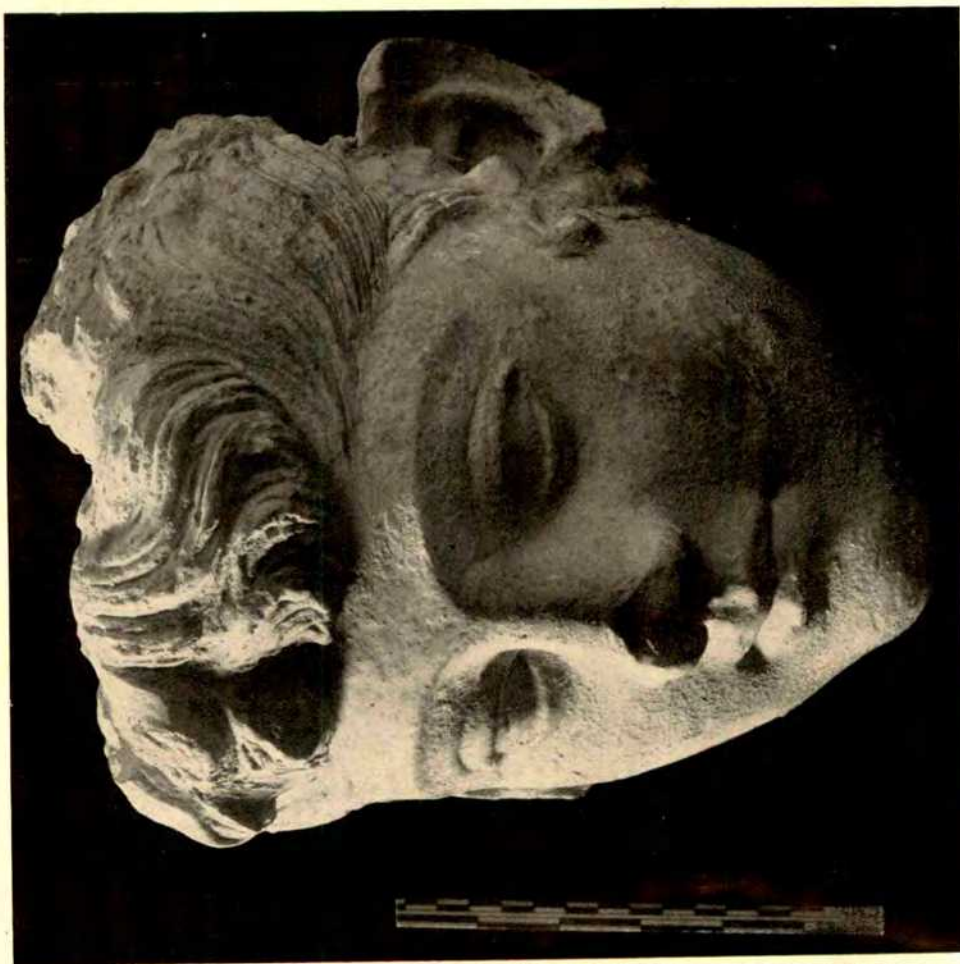


a. HEAD OF THE APOLLO-BELVEDERE
(Rome: Vatican)

PLATE II



d. BUST OF THE YOUNG MARCUS AURELIUS c. A.D. 150
(Rome : Capitoline Museum)



b. STUCCO HEAD FROM THE DHARMARAJIKA MONASTERY, TAXILA
Height, 10½ inches

PLATE III



a. STUCCO HEAD FROM THE JAULIĀN MONASTERY, TAXILA
Height, 4½ inches



b. STUCCO HEAD OF SATYR FROM TAXILA (SIRKAP)
Height, 8½ inches

PLATE IV



a. HEAD OF GAUL
(Rome : National Museum)



b. STUCCO HEAD FROM THE NORTHWEST FRONTIER, PAKISTAN
(Eric Dickinson Collection). About half life-size

PLATE V



A



B

a. SCHIST FRIEZE FROM THE KUNĀLA MONASTERY, TAXILA

Length, 15 inches

b. STUCCO (left) AND STONE (right) SCULPTURES FROM HADDĀ, AFGHANISTAN
(Délégation Archéologique Française en Afghanistan)

PLATE VI



a. STUCCO BUDDHA IN THE JAULLIAN MONASTERY,
TAXILA:
c. 4th to 5th cent. A.D. About $\frac{3}{4}$ life-size



b. STUCCO BUDDHA IN THE JAULLIAN MONASTERY, TAXILA:
c. 4th to 5th cent. A.D. Compare Plate VII *a*
(Scale is of inches and centimetres)

PLATE VII

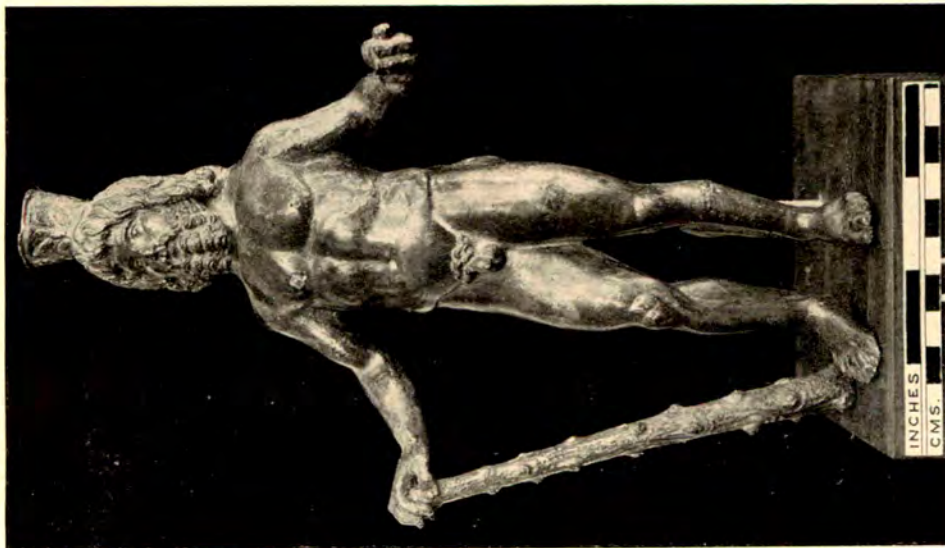


b. STUCCO HEAD SHOWING TWO PERIODS OF WORKMANSHIP, FROM SAHR-I-BAHLOL,
NORTHWEST FRONTIER PROVINCE, PAKISTAN (PATNA MUSEUM)
About half life-size



a. HEAD OF STUCCO BUDDHA SHOWING TWO PERIODS OF WORK-
MANSHIP ; IN THE JAULIAN MONASTERY, TAXILA
Compare Plate vi b. Height, 6 inches

PLATE VIII



b. BRONZE STATUETTE OF HERAKLES-SERAPIS, FROM THE BEGRAM HOARD, AFGHANISTAN (Kabul Museum). Height, 9½ inches



a. STUCCO BODHISATTVA OF 'ANTINOUS' TYPE, FROM HADDA, AFGHANISTAN (Délégation Archéologique Française en Afghanistan) About half life-size

PLATE IX



A



B



C

a, and b. STUCCO PLAQUES FROM THE BEGRAM HOARD, AFGHANISTAN
(Kabul Museum). Diameter, about 6 inches

c. STUCCO MOULD WITH IMPRESSION (right) FROM THE ROMAN CITY OF SABRATHA,
TRIPOLITANIA, N. AFRICA
(Sabratha Museum). Diameter, 7 inches



A

PLATE X

- a. STUCCO BODHISATTVA, FROM THE JAULIÂN MONASTERY, Taxila
Height of Head, 7½ inches
- b. STUCCO HEAD OF BODHISATTVA, FROM HADDA, AFGHANISTAN
(Délégation Française en Afghanistan)
- c. STUCCO HEAD FROM THE ROMAN CITY OF SABRATHA, TRIPOLITANIA, N. AFRICA
(Sabratha Museum). Height, 5 inches



B



C

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY

let him see his books. Then Columba prayed that no man alive might be able to read these books and from that time forth not a word of them could be read. Another story, more complimentary to Lon Garad, tells that on his death the books in all Ireland fell from their satchels and that learning in Ireland suffered a terrible loss. It has been suggested that the books contained druidical knowledge, but the references to Lon Garad are too respectful. Is it not possible that Lon Garad spoke and wrote an unintelligible language and perhaps practised some regional and unorthodox cult? When the stories of St. Columba were being developed in the Columban monasteries, tales would be woven round those occasions when their monks in other parts had met with some intransigence of faith or language and when they had either overcome it or respectfully come to terms with it.

St. Canice, who is definitely associated with the older races, is mentioned constantly in connexion with books. He had two conflagrations in his monasteries. On one occasion his gospel-book, on the other his whole library was miraculously preserved. Another time the gospel-book was left out in the snow and yet reappeared miraculously undamaged. It is told that, when he was studying in St. Cadoc's school in Wales, at the master's bidding he abandoned his copying work with exemplary obedience in the middle of the letter o (41). This story may derive from some unfinished or half destroyed ms attributed to him. Particularly puzzling and interesting is the story of the gospel-book, which St. Canice wrote in 40 days and 40 nights on an island in Lough Cree near Roscrea. It was called 'Glass Kannechi', which was long interpreted as Canice's Fetter, a curious name for which several not very satisfactory explanations have been given (42). I think his successors and biographers were as much puzzled by it as we are. Otherwise I cannot explain the queer-story of the abböt, who succeeded him at Aghaboe (43).

This Abbot, Liber, had been a bad young man, who despised the Word of God and therefore died from a fall from a horse. St. Canice resurrected him and he did penance, but Canice trusted him so little that when he took him to Britain, he kept his legs bound with fetters and threw the key into the sea. After 7 years he was allowed to return to Ireland, and he found the key to his fetters in the stomach of a fish which he caught in the Liffey.

What does this story mean? Surely the fettered St. Book, whom St. Canice brought about with him to Britain and back, must have some connexion with St. Canice's holy Book, which was called his Fetter. Is it not possible that St. Canice's book inherited the veneration, which was due to its supposed writer? Simple people, hearing of Holy Liber, which was to be found in a certain monastery, may, in their ignorance of Latin, have supposed that some holy saint was meant. So St. Book would find his way into the Calendar. St. Book, like St. Canice, was associated with Britain, so may not the book have been written in the British tongue or in any case in a language that the simpler Gaels would deem foreign? The lives of the saints are of course very late, and the book attributed to St. Canice may well have dated from many centuries later than the date claimed for it. But the book and the legends associated with it must have presented a problem to the hagiographer, which he had to solve as best he could. His story is so queer that some explanation of the kind I have offered must be made.

There is another story of a destroyed library, connected with Ossory. St. Moling, fearing for the loss of his books during an invasion, entrusted them to St. Muicin, who

⁴¹ *V.S.H.* I, 153.

⁴² *V.S.H.* I, 167 and II, 386.

⁴³ *V.S.H.* I, 157.

ANTIQUITY

lived at Mayne, near the Caves of Dunmore, north of Kilkenny. He hid them in the caves, where they were destroyed by the floods. St. Moling forgave him. St. Moling was the patron saint of the Lagen and the disappearance of his books in Ossory may be a way of describing the disappearance of a Lagen remnant, their cult and language.

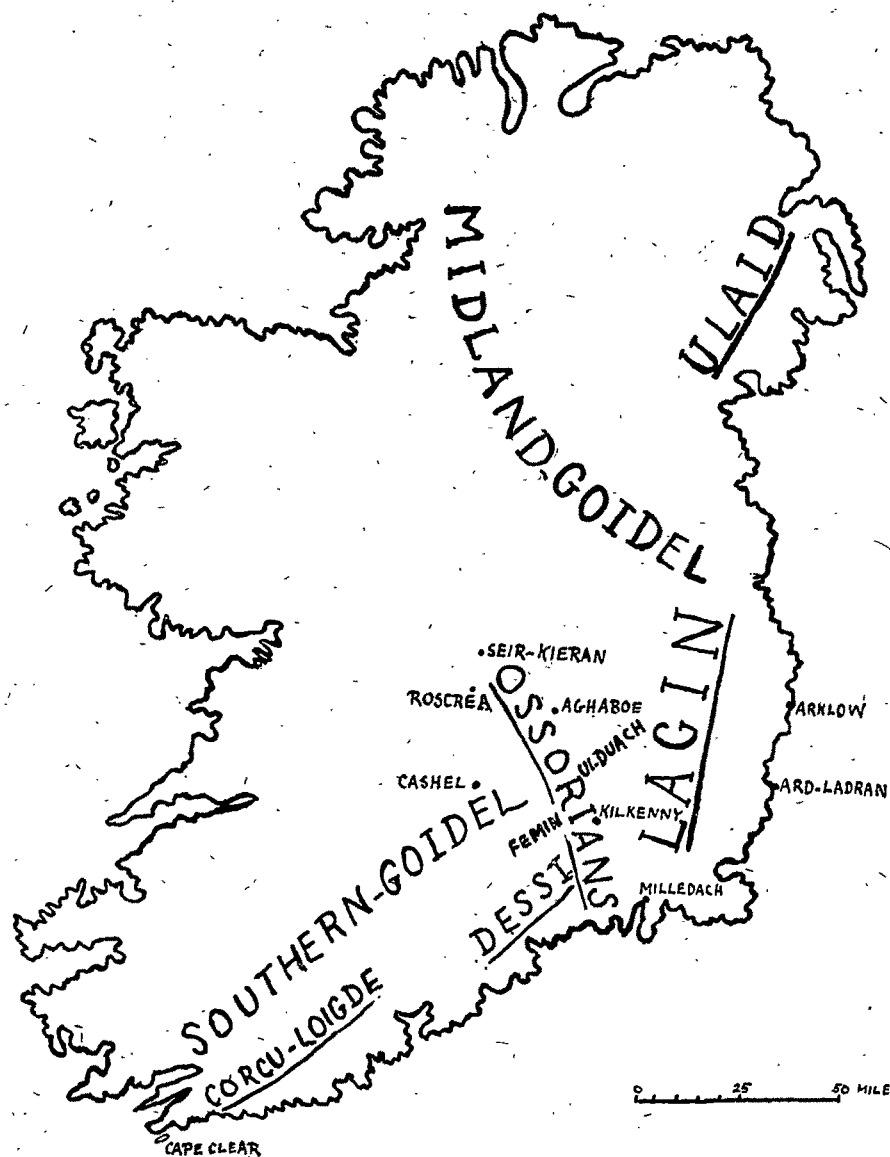
Whether these conjectures are wide of the mark or not I shall not apologize for them. If you are digging for treasure, even your failures are illuminating to the next comer. The lives of the saints were once treated very reverently, and local scholars, such as Hogan and Carrigan (44), travelling round the country parishes, tried to bring local legends, ruins, earth-works and place-names into conformity with these imaginary histories. Later on too great a reaction set in, the lives of the saints were pronounced nonsensical, and Hogan and the others were discarded as naïve and unreadable. The work of Charles Plummer has now, through its careful, relentless analysis, reinstated the lives of the saints as wonderful source-books for the interpretation of the past.

The hagiographers, whose daily life was full of chaos and tragedy, naturally preferred uniformity above everything; they relished the stereotyped anecdote and crude standardized virtues and vices. They liked history to have a sharp simple pattern and had no taste for the curious anomalies, the divergences between fact and theory, which to us are more revealing than their smooth moralizing. They did their best to suppress the evidence of disharmonies in religious cults, language and race. They did not like it to be known that many of their most sacred practices had very uncouth origins. All the same they were not always successful in suppressing what displeased them, and it is these failures of theirs which are most enlightening to us.

Plummer's critical apparatus was derived from the library and the study, O'Donovan's, Hogan's and Shearman's far more from the open fields. I think fresh progress would be made, if the investigation of these problems was pursued in the spirit of Plummer but with a new approach. We should start again from the close examination of the countryside and its legends. For example the deductions of the 19th century historians of Ossory are obviously almost all wrong, but the facts which they accumulated and collated, are invaluable. There are more facts still to be collected and brought into relation to modern scholarship. When this has been done, it will be easier to write the history of the Irish tribes and the slow, haphazard introduction of Christian beliefs.

⁴⁴ Canon Carrigan's *History of Ossory* is still an indispensable work but it is marred throughout by his too great refinement. For example, he averts his mind from the curious diet of small boys on which the Dessi nourished Ethne and writes, 'Under their assiduous care, she grew up to become eminent for ability as well as beauty'. op. cit. I, 29.

THE DUMB AND THE STAMMERERS IN EARLY IRISH HISTORY



CELTS ARE UNDERLINED

The Excavations at Cairnpapple Hill, West Lothian 1947-8

by STUART PIGGOTT

IN ANTIQUITY 1948, p. 35, a brief account was given of the first season's work on a Bronze Age sanctuary and burial site on Cairnpapple Hill, near Torphichen in West Lothian. With the co-operation of the Ancient Monuments Department of the Ministry of Works, excavations were continued in the summer of 1948. The site was completely stripped, and revealed a complex series of structures indicated by the sockets of once-standing stones or by stones still extant. It will be laid out and conserved by the Department as an Ancient Monument under guardianship. The following account of the main results of the 1947-8 excavations is intended as a preliminary to the full excavation report, which will appear in due course in the *Proceedings* of the Society of Antiquaries of Scotland, who financed the first season's work.

Cairnpapple Hill, the summit of which is within the 1000 feet contour, is a part of the Bathgate Hills, which form a compact block of high land between the main road to Stirling on the north and from Glasgow to Edinburgh on the south. On the summit, the site before excavation was chiefly distinguished by the grass-grown cairn which gives its name to the hill, but most maps and the earlier antiquarian literature indicated a 'fort' on the same site. Field-work in 1946 had shown that the cairn stood eccentrically within a low roughly circular earthwork (the 'fort') which on surface showing was almost certainly a member of the 'Henge Monument' class of structure. The site was confused by an octagonal turf dyke which had been made round the cairn in the late 18th or early 19th century to enclose a plantation of trees, now vanished.

The excavations in 1947 showed that the surface survey did indeed represent the actual state of affairs, and that a Henge consisting of an external bank and internal rock-cut ditch, surrounding a circle of stone-holes, was overlaid by a later cairn set within the area, but west of its approximate centre. It was therefore clear that the further excavation would involve two separate problems—the stripping of the whole area of the Henge within its ditch, and the complete excavation of the large cairn which though robbed of stones in many places seemed essentially intact. Both operations were carried out in 1948, and the site was found to contain further complexities of structure, involving in all at least five phases of ritual or burial performances on the hill-top. Fortunately the relation of one phase to another was in almost every instance stratigraphically fixed, and although small finds were few, they served to date the structures and to give a coherent chronological framework. The accompanying plan (FIG. 1) shows these features as finally revealed.

PHASE I

The first monument erected on the site appears to have consisted of an irregular arc of standing stones, facing west and with a chord of about 55 feet. In or against all but one of the seven stone-holes were deposits of cremated human bone, and five additional similar deposits were found in the immediate area of the arc, two carrying on its line westward at the north and south ends. No stones remained standing or

CAIRN PAPPLE HILL WEST LOTHIAN

DIAGRAM-PLAN OF EXCAVATIONS 1947-8

STONE-HOLES: PROBABLY NEOLITHIC ○ BEAKER PERIOD ●
 STONES: BEAKER PERIOD / FOOD-VESSEL PERIOD ■ MIDDLE BRONZE AGE
 CREMATIONS *

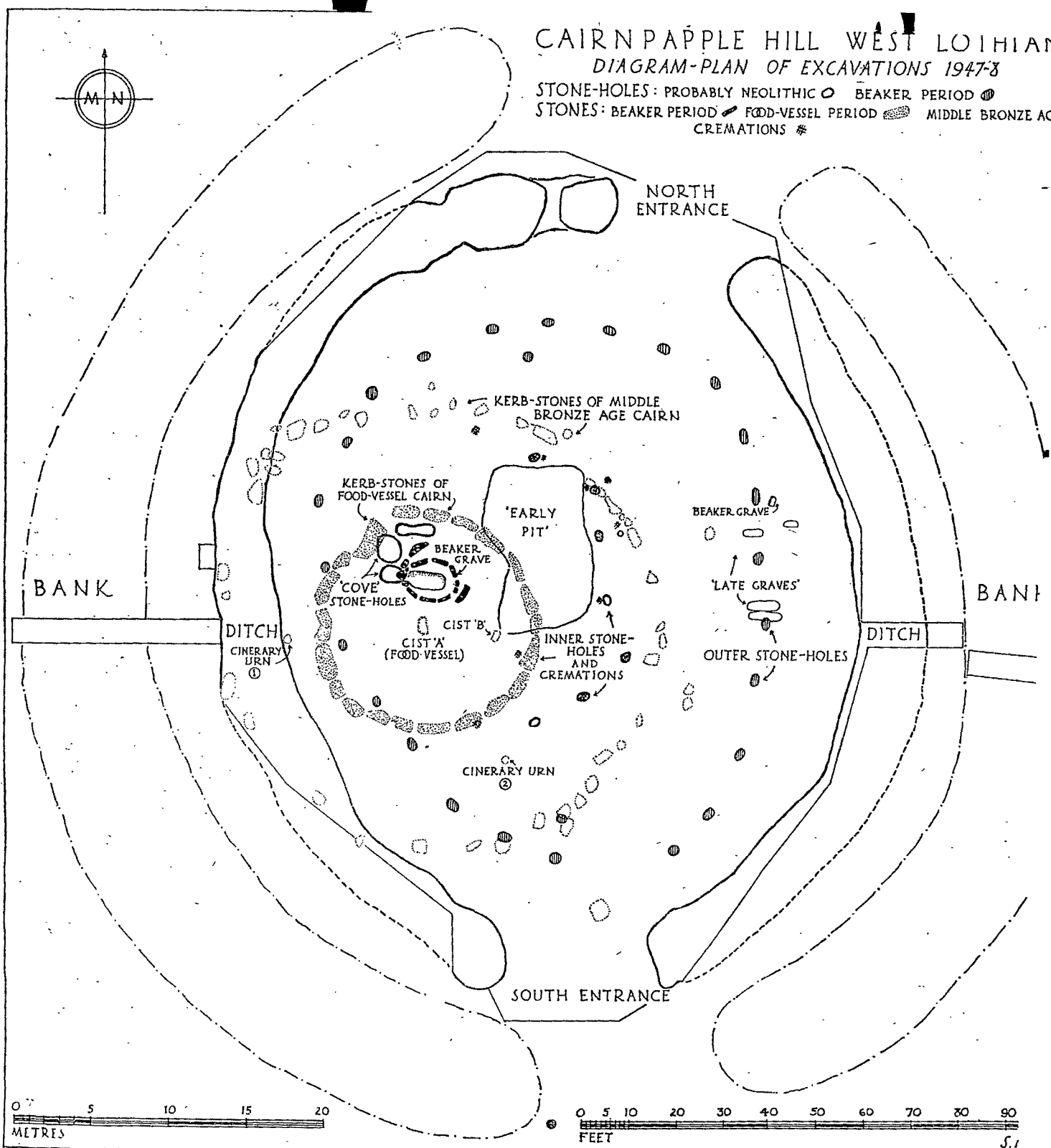


Fig. 1

THE EXCAVATIONS AT CAIRNPAPPLE HILL, WEST LOTHIAN 1947-8

identifiable, and stratigraphically the arc could be dated only by the fact that the stone-holes were beneath a late Middle Bronze Age enlargement of the main cairn: two cremations were however overlaid by the earlier phase of the cairn, of Food Vessel date.

By analogy, however, stone-holes with cremations in or by them should belong to the class of monuments represented by those recently excavated near Dorchester-on-Thames (with wooden uprights) and the first phase of Stonehenge (ditch, bank, Aubrey Holes and cremation-cemetery). With one of the Cairnpapple cremations was a broken bone pin of a type exactly matched at Stonehenge and Dorchester, and in several late Neolithic contexts elsewhere. There is good reason for thinking that the Dorchester sites, and Stonehenge I, are of late Neolithic date and affiliations, and to this period I would assign the Cairnpapple arc and its cremations. On the old ground surface under the Food Vessel cairn were found two pieces of polished stone axes, petrographically identified as being products of the Graig Lwyd and the Langdale Pike axe-factories respectively, and a sherd of Western neolithic pottery, while another similar sherd came from the filling of a Beaker grave nearby—all these might plausibly be connected with Cairnpapple Phase I. There is no evidence when the stones were removed from the stone-holes, save that this had taken place, and the holes had become filled up, before the end of the Middle Bronze Age.

Opposite the open end of the arc, though not quite central to it, were three huge stone-holes which had become deprived of their stones and filled up by the time the Food Vessel cairn was built. Furthermore, the southernmost of these stone-holes had been re-used for a smaller stone than that for which it had been dug, forming the end-stone of a setting round a rock-cut grave of Beaker date. The setting of three large stones, then, must be pre-Beaker in date, and so likely to be contemporary with the arc. They can only be interpreted as having held the stones of what Stukeley called a 'Cove', and for which no alternative name has ever been put forward. Such 'coves' exist within the North Inner Circle at Avebury and (freestanding, as one assumes that at Cairnpapple to have been) at Stanton Drew in Somerset. It is more than probable that there was a similar structure within the Arbor Low Henge, now represented by fallen stones.

Phase I at Cairnpapple then seems to have consisted of a Cove of three large standing stones, facing east, and an Arc of smaller stones with the open end towards the Cove and the west. Deposits of cremated human bones had been made in and around the area of the Arc. The date of this phase should be pre-Beaker, and is likely to have late Neolithic connexions.

PHASE II

Here, fortunately, the evidence is clearer and the structures less unusual, though nevertheless there are some surprising features. The main elements of the phase are the bank, ditch and stone-holes of a Henge Monument very similar to Arbor Low. The bank measures 200 feet crest-to-crest on its east to west diameter, and within it is a quarry-ditch separated from it by a marked berm. The ditch is broken by two entrances, roughly north and south and 30 feet wide, and encloses an oval area 145 by 125 feet. It is irregularly dug in the rotted rock subsoil and at the time of excavation was silted up nearly level.

Within the area enclosed by this ditch was an oval setting of standing stones, represented by stone-holes with packing-blocks in most instances. The oval measures 115 by 92 feet and consists of twenty-four stone-holes forming the main setting, with two 'inliers' to north and south, making up twenty-six stones in all. The spacing

ANTIQUITY

between stones is not quite regular, but averages about 14 feet, and although the stones continue in even spacing across the northern entrance through the ditch, at the south there is a gap of 25 feet constituting an entrance to the stone setting nearly opposite that in the ditch. There was no evidence (in the form of stone chips, etc.) to suggest that the stones had been destroyed in or near the stone-holes, but they had been bodily removed.

Against one of these stone-holes on the east was a rock-cut grave for a crouched inhumation, completely destroyed by the acid soil, but accompanied by a surviving beaker of class C. No other graves were found associated with the stone-holes, but scraps of undecorated Beaker pottery were found in the silt of the ditch on the west.

Within the Henge area to the west, however, was found a remarkable burial of Beaker date (PLATE I). In the southernmost of the three stone-holes of the Cove an upright stone, nearly eight feet high, had been set in such a way as to occupy only the easternmost part of the large hole: there were no packing blocks and the hole was filled with rubble only. This stone formed the westernmost (and by far the largest) member of an oval setting 11 by 9 feet, the other stones being set up on or just in the old surface, and enclosing a large grave 7 by 4 feet dug in the rock. Outside the oval setting three stones appeared to have formed part of an outer ring. The whole of these features had been incorporated within the Food Vessel cairn, which had originally covered even the standing stone, though at the time of excavation the top of this projected through the robbed cairn surface.

In the grave was found evidence of an inhumation at full length. The body was represented only by a slight staining on the rock floor of the grave, and by the fragmentary remains of the enamel crowns of the teeth 5 feet 6 inches from the standing stone, which must have stood at the foot of the burial. These teeth were found under, and partly embedded in, an area of carbonised oak wood about 8 inches across and three-quarters of an inch thick, which must have been lying over the face of the corpse. On the north side of the grave were the broken remains of two beakers, one at the foot of class C and the other (of class A) above the position assumed for the head of the interment. Between them, along the side of the grave, lay the carbonised remains of a large oaken object, perhaps a club, 3 feet 6 inches long, expanding to a width of nearly one foot at the upper end. A layer of carbonised oak wood was also found over the beaker at the foot of the grave.

The whole burial is very remarkable: the full-length inhumation, the two beakers and the wooden objects all being unusual features. The wood over the face is even more curious and difficult to explain, but one cannot wholly dismiss the possibility that it represents the remains of a wooden mask worn over the features of the deceased. Such an elaborate burial within the area of the Henge, and against the site of the former Cove, can hardly be classed as anything but ritual in secondary, if not primary, intention. So far as the Henge can be claimed to have a ritual or liturgical centre, this burial must occupy the most likely position for such a focus.

It seems clear that the grave and its oval setting with tall foot-stone must have been incorporated within a small cairn as a part of its original structure. The setting could not have stood free without some support and it could not have taken the thrust of a small cairn within its circumference, but the two larger stones outside the oval setting may well have formed part of such a kerb. Direct evidence of the former presence of such a mound should have been visible in section within the later Food Vessel cairn, but by an unfortunate chance that part of the cairn had been heavily robbed, and such stratification as remained was confused and disturbed.

THE EXCAVATIONS AT CAIRNPAPPLE HILL, WEST LOTHIAN 1947-8

Eastwards of this grave was a large shallow pit dug in the rock to a general depth of not more than a foot, but going down into oval scoops up to two feet deep in three places. This seems only to be explained as a quarry: the filling showed that it had been left open to silt naturally from the outer edges, and when the cairn was built partly over this pit in Food Vessel times, it was deliberately filled up level for the required distance. Two scraps of undecorated Beaker in the silt show the pit to be approximately of this date, and one can only tentatively explain it as the quarry from which the rubble to make the small cairn over the Beaker grave and its stone setting just described.

To sum up: Phase II saw the building of a Henge Monument similar in many respects to that of Arbor Low, enclosing the older Arc and Cove. There is no evidence to show whether the Arc stones were still allowed to stand, but it does seem inevitable that the southern Cove stone at least must have been removed from its socket before the foot-stone of the Beaker grave was set up—although both could conceivably have stood touching each other in the capacious stonehole. The whole of the Henge structure, and the surely ritual burial in its little cairn, is securely dated by the three Beakers found. It is difficult to understand why the ritual burial was so 'off-centre' to the west: the presence, actual or ritual, of the Cove seems the only explanation, with a desire to re-hallow the older liturgical centre when the later monument was erected.

PHASE III

With the third phase at Cairnpapple comes a significant change of intention. The interest shifts from the ritual to the funerary aspect, and a large burial cairn is built on the site within, and largely at the expense of, the earlier structures. The cairn visible before excavation was found on excavation to be of two dates, the second an enlargement to twice the diameter of the first mound (PLATE II).

The earlier cairn had an approximate diameter of 50 feet and was bounded by a massive kerb made of large stones laid on their sides on the old ground surface. These stones, like all others surviving on the site, are of local origin and could have been dragged from anywhere on the hill-top or nearby, but in view of the complete disappearance of the Henge stones, it seems extremely likely (though of course unproven) that they were taken down and re-used to form this cairn kerb. There are twenty-one stones in the kerb, and taking into account the Cove, the Arc and the Henge oval, there could have been thirty-six stones to choose from. There is no doubt that some at least of the Henge stones must have been removed at or before the time of building the cairn, for it overlies their filled-in stone-holes on the west, as it does those of the Cove. On the whole it seems most probable that the Henge was depleted, if not destroyed, to form the kerb of the cairn and perhaps some of its internal structures.

The precise position of the cairn seems to have been determined by a necessity felt by its builders for incorporating within its kerb and mound the earlier Beaker grave. This involved building part of the cairn, with its heavy kerb-stones, over the quarry-pit east of this grave: the pit seems to have been open with the exception of a small deposit of silt against its outer edges, and on its western edge, where the kerb overlies it, it was filled up deliberately with stiff blue boulder-clay brought from the foot of the hill.

The cairn was mainly built of large stones without earth, though on the east and south a thick layer of the same blue clay as that used to fill the pit was incorporated in the body of the cairn between two layers of stones: it had been trampled down and iron-pan had formed on it, as it had over that rammed down into the pit. But the precautions taken were in vain. The kerb over the made soil in the pit gave way, and the whole body of the cairn slipped disastrously to the north-east, spilling out over and partly

ANTIQUITY

into the pit, which seems, however to, have been filled or silted by the time the collapse took place.

Centrally within the kerb was a large cist, constructed by setting upright slabs to line a rock-cut pit, and then building up horizontal dry-stone walling to take the single massive capstone. The collapse of the cairn north-eastwards had caused this cap to be wrenched aside, and the walling and cairn material on the west had tumbled in, half-filling the cist. When the situation was observed during excavation, little hope was held out of intact or even recognizable grave-goods beneath this tumble of heavy stones, and it was with considerable astonishment that an intact Food Vessel was found lying among the uppermost stones in the cist. Below the stones, smashed fragments of unburnt human bones were found on the floor of the cist, pounded to smears and scraps. The intact pot on top of the filling was difficult to understand until a more careful study of the construction of the cist walls showed that on the east wall, immediately adjacent to the spot where the Food Vessel was found, the stones had been set back so as to form a shelf on which the pot must have stood, to fall only a few inches when the cairn material shifted and the upper part of the western wall of the cist collapsed inwards.

Between this cist and the kerb on the east, was another with a very large and heavy capstone, but built only of small boulders enclosing a rectangular space only eight inches deep and containing in its centre an unaccompanied human cremation. There seems no doubt of the contemporaneity of the two cists, and the whole cairn must be of Food Vessel date.

In the stones filling the central cist was one squarish block, almost certainly part of the walling on the west, with three cup-marks 'pecked' deeply on one face. Elsewhere scattered in the body of the cairn were three or four other stones bearing each a shallow 'pecked' cup-mark.

Phase III marks the first specifically funerary use of the site, with a large cairn having a central cist containing a Food Vessel inhumation, a second cist with a cremation, and a massive kerb almost certainly formed of the re-used stones of the Henge Monument, whose destruction probably dates from this time.

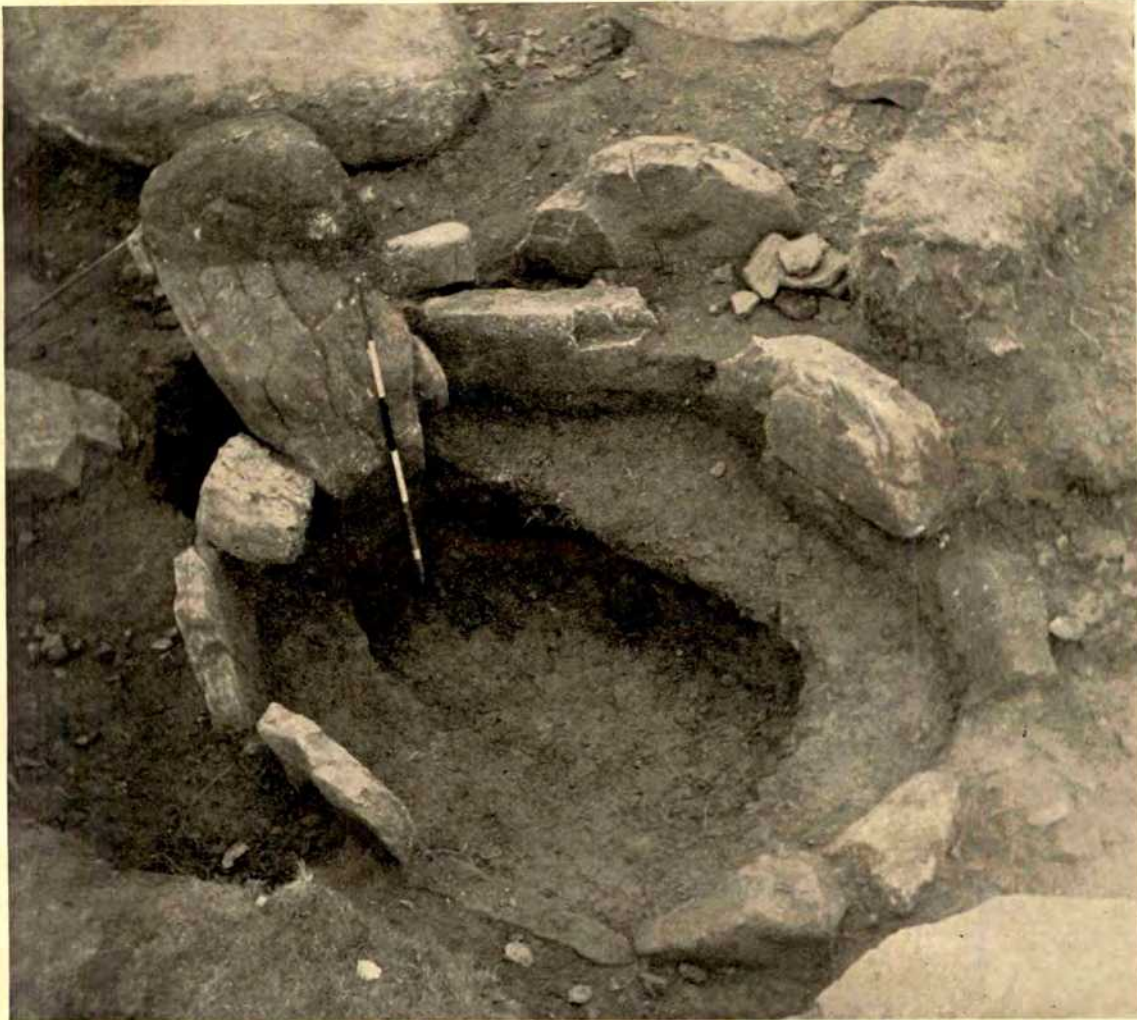
PHASE IV

The kerb of the Food Vessel cairn was completely invisible on the surface when the excavations were begun, though an irregularly circular kerb of boulders with a diameter of about 100 feet was conspicuous round the periphery of the visible mound. It was apparent at an early stage of the work that this outer kerb was that of an enlargement to the original, 50-foot, cairn, in which the mound had been increased to twice the diameter, though probably with little addition to the height, of the earlier structure.

This enlargement consisted of smaller stones and a greater proportion of rubble and earth than the Food Vessel cairn, and on the west it overlay the now silted-up ditch of the Henge for a distance of over 80 feet of its inner edge. The kerb was of smaller and rounder boulders than the long stones of the earlier tomb, and these were irregularly placed on the old ground surface without any form of bedding or packing into place. Within the circuit of this kerb, and under the mound of the enlarged cairn, were the Arc stone-holes of the first phase, as well as the quarry-pit probably of Phase II, and seven stone-holes of the Henge in addition to two more under the Food Vessel cairn as well.

Two burials referable to this enlargement were found, one on the west and one on the south, and both in inverted Overhanging Rim cinerary urns. The west burial (no. 1) was in a shallow pit in the old surface, and the pot had collapsed and telescoped

PLATE I



CAIRNPAPPLE HILL: BEAKER GRAVE WITH STONE SETTING OF PHASE II, INCORPORATED WITHIN
FOOD VESSEL CAIRN OF PHASE III

PLATE II



CAIRNPAPPLE HILL; CAIRN COMPLETELY EXCAVATED, SHOWING KERB AND CISTS OF PHASE III (FOOD VESSEL)
AND GRAVE OF PHASE II (BEAKER). (Looking east)

THE EXCAVATIONS AT CAIRNPAPPLE HILL, WEST LOTHIAN 1947-8

over the cremated human bones. There were no grave-goods except a large pin of Red Deer antler, also burnt. The second burial, on the south, was similarly in a shallow pit, but the pot was almost intact, inverted over cremated human bones with a burnt bone pin with eyed head. A feature of interest however was that in the shallow pit and against the sides of the urn had been placed a quantity of dark soil containing fragments of charcoal, tiny sherds of cord-ornamented pottery, and fragments of flint implements splintered by fire. The whole appearance was strongly suggestive of soil from a hut floor and hearth. Both urns are typologically late in the cinerary urn series, and should on conventional nomenclature be late Middle Bronze Age.

Phase IV, the enlargement of an earlier burial mound for the deposition of new burials, with the provision of a new outer kerb, follows a well-known Bronze Age precedent in this country as well as on the Continent. It implies continuity in tomb-building and a continued veneration of an ancient burial.

PHASE V

The final phase at Cairnpapple is curious, and not easy to understand. On the eastern side of the Henge, four graves were found cut in the rock, one destroying the greater part of a stone-hole of the Beaker period monument. There was no trace of skeletons or grave-goods in them, but they were clearly intended to accommodate bodies at full length. Their relationship to the stone-hole shows that they cannot be earlier than the phase at which the Henge stones were removed from their sockets—presumably Food Vessel times—and their presumptive purpose for full-length inhumations in a small group or cemetery suggests a date late in the prehistoric period. Elongated graves are known in North Britain in the Iron Age, and within this rather vague period covering the first few centuries before and after Christ the Cairnpapple 'Late Graves' may be placed. Although they are orientated east and west, it is very improbable that Early Christian graves would be made on a remote hill-top within an ancient pagan sanctuary.

The Cairnpapple excavations have revealed a remarkable sequence of structures related to religious or funerary rites on the same site. Continuity over a thousand years is virtually proven, from the late Neolithic to the end of the Middle Bronze Age: if the Late Graves are indeed Iron Age this would imply that after an interval of many centuries the site was still regarded as a sacred place suitable for burial at the dawn of the Christian era. For a parallel in continuity we have to turn to Stonehenge, spanning almost exactly the same period, from the late Neolithic Aubrey Holes to the Iron Age z and y Holes of the 1st century B.C.

Continuity in barrow-building, with the enlargement and rebuilding of the cairn and its related features, has become well-known to us since the brilliant excavations of Sir Cyril Fox in Wales, but the mixed religious feelings that could desecrate a Henge by taking down its standing stones, and yet carefully build a tomb within its area in such a way as to incorporate one of the earlier features, seem unattested elsewhere in Britain. The relationship of the Food Vessel people to those of the Beaker culture, at Cairnpapple at least, was not one of direct continuity. Whatever part the Henge played in the spiritual life of the Beaker folk in the Lothians, the Food Vessel people approached it from a different point of view. Perhaps it possessed a certain sanctity, enough to render it a desirable place for the burial of a chieftain, but its standing stones were regarded as nothing more than a convenient quarry for kerb-stones.

Not only is the sequence and the continuity of Cairnpapple arresting and unusual, but the individual elements in the various phases of building and reconstruction have

ANTIQUITY

peculiar interest. The Arc of Phase I is a feature without apparent parallels in Britain, but it sets us on our guard against accepting all allegedly half-destroyed stone circles as incomplete monuments. The cremations, and the characteristic bone pin found with one of these, add to the rapidly growing list of cremation cemeteries that can be shown to be pre-Beaker, and provide welcome confirmation of the connexions already suspected between the cultures of Skara Brae and Ronaldsway in North Britain and those represented at Dorchester-on-Thames, Woodhenge and Stonehenge in the south (1). The two axe-fragments of imported rocks, if they belong to this phase, stress the southern connexions.

The finding of the stone-holes of an authentically Stukeleian 'cove' in Scotland was an unexpected as it is important. Here again is a link between Wessex and the Forth basin—the only extant 'coves' are at Avebury and Stanton Drew: undated at the latter site and at least no later than Beaker at the former. It seems almost certain that the now prostrate stones in the centre of Arbor Low are another such 'cove', in ruin, and one recalls that by these stones an extended inhumation burial was in fact found in Mr St. George Gray's excavations (2). Here Cove and Henge seem parts of an indivisible whole, but at Cairnpapple the Cove seems the earlier feature.

But Arbor Low certainly provides the parallel for Cairnpapple in Phase II. The Derbyshire Henge is not only geographically the nearest, but in its general proportions it closely resembles Cairnpapple, though slightly larger, and significantly enough the stones, so far as can be judged from their present fallen condition, were set in an oval and not a circle—a curious feature shared, incidentally, by the posts at Woodhenge. Arbor Low, too, is one of the few Henge monuments set on a hill-top in such a way as to command a wide view, and at Cairnpapple this dominating position is such that from the site can be seen the Bass Rock on the east, and the mountains of Arran on the west, while to north and south the view is bounded by the Highlands and the Lammermuirs.

The construction of Phase II at Cairnpapple gives one the immediate impression of the work of people unaccustomed to a land of hard rocks. It was indeed lucky for its builders that the rock of the hill-top was sufficiently rotted to be, in its way, not much more difficult than hard chalk to dig into—the digging of the Cairnpapple ditch cannot have been such a task as the sheer quarrying in the limestone necessary at Arbor Low. But at Arbor Low the stones seem to have been set up in the shallowest of sockets, if these were dug at all, whereas at Cairnpapple the stone-holes were always well-cut and often surprisingly deep. The two graves containing Beaker interments, too, were dug in a manner peculiarly reminiscent of graves of similar date in southern England, and seem to be unique in Scotland, where the stone-slabbed cist is ubiquitous among the recorded Beaker burials.

In both Phase I and Phase II, in fact, connexions with the south are apparent. But with the change to funerary intent on the site, and the building of the Food Vessel cairn of Phase III, we are clearly dealing with a people familiar with the country and building in the traditions proper to it. If, as seems inevitable, the Henge was dismantled in this phase, it can only be as the result of a decisive break in religious tradition. If the Food Vessel folk have claims to be regarded as the inheritors of an ancient Neolithic culture within the Peterborough family (as their pottery and certain of their flint types so strongly suggest), then at Cairnpapple at least they are establishing themselves after a relatively short period of Beaker domination has come to an end. The Food Vessel

¹ Cf. Dr J. F. S. Stone's analysis in *Antiq. Journ.* xxviii (1948), 149-56.

² *Arch.* LVIII (1903), 461-98.

THE EXCAVATIONS AT CAIRNPAPPLE HILL, WEST LOTHIAN 1947-8

cairn built on top of the Henge bank at Arbor Low suggests a similar state of affairs. Like the early Christian missionaries, they destroyed the *fana idolorum*, but re-consecrated the site to their own ends, and, pulling down the old temple, used its stones to wall in the tomb of a chieftain built over the dedicatory burial of the earlier and alien shrine.

The enlargement of this cairn follows a practice familiar to us from many sites in both the Lowland and the Highland Zones of Britain. Barrow IV at Beaulieu (3) in the New Forest is a good example from the south, with an original ditched mound of turves, 40 feet in diameter and covering a Food Vessel burial, later enlarged to 85 feet in diameter at some undetermined period before the Early Iron Age. At Sutton, Glamorganshire, (4) and Talbenny, Pembrokeshire (5), barrows of the Beaker period were enlarged to accommodate each a single Middle Bronze Age cremated interment: at the latter site the new kerb was made by removing the stones from the earlier mound, and Fox comments on 'the decay of the fine traditions of craftsmanship in the structural use of unwrought stone . . . as the Bronze Age developed'. Such a remark is very apposite when comparing the two kerbs at Cairnpapple, with the massive carefully-placed stones of the Food Vessel cairn contrasting with the haphazard collection of boulders that grace the edge of the Middle Bronze Age enlargement.

The two cremated burials call for little comment, except for the deposit of occupation soil made over and around the pot in No. 2. This feature recalls the deposit of 'occupation soil' recognized by Phillips in the Giants Hills Long Barrow in Lincolnshire (6), and may well have occurred elsewhere in Bronze Age burials, and lurk disguised under the familiar 'black unctuous earth' of early barrow-digging accounts.

If the graves of Phase v are to be attributed to the Iron Age, one can only note, with all reserve, other evidence of an interest in Henge Monuments at this period to which Mr C. E. Stevens first drew attention (7), and of which Stonehenge affords the most striking example. The Geographer of Ravenna, copying, in the Dark Ages, lists of names off a map of the Roman Empire, included among those upon or near the line of the Antonine Wall, *ubi et ipsa Britannia plus angustissima de Oceano in Oceano esse dinoscitur*, a *Medio Nemeton*—the Middle Sanctuary (8). Standing on Cairnpapple Hill on a clear day, with a view that virtually does stretch across Britain from sea to sea, one is tempted to wonder whether the two-thousand year old shrine still retained some vestiges of sanctity among the Iron Age Celts who may have been buried within its hallowed circuit.

³ *Proc. Prehist. Soc.* ix (1943), 1-27.

⁴ *Arch.* LXXXIX (1943), 89-126.

⁵ *Arch. Journ.* xcix (1943), 1-32.

⁶ *Arch.* LXXXV (1936), 37-106.

⁷ *Oxoniensia* v (1940), 166-7. Cf. *ANTIQUITY* xv (1941), 305-19.

⁸ *Cosmographia*, ed. Pinder and Parthey, Berlin 1860, 434-5. The identification of the sites, ten in number, said to be *recto tramite una alteri connexae* is by no means easy. Cf. Macdonald, *Roman Wall in Scotland* (2nd Edn. 1934), 189-90, for the problem.

Roots and Origins : a review*

by E. G. R. TAYLOR

THE late Mr Thomas Burke was well known for his popular writings on London life, and the phrases employed by literary reviewers of his earlier works are precisely applicable to his *English Townsman* : 'it swarms with rare and amusing pictures,' 'it is a mine of out-of-the-way facts.' A man of strong prejudices, Mr Burke was accustomed to see what he expected to see, factory workers, for example, to this day commencing toil at 6 a.m. while their employers 'roll-up in their cars at about eleven.' Nevertheless he is concerned to emphasize the wholly laudable thesis that it is just as 'natural' for men to live in towns as in villages. Yet his supporting arguments are not always very happy, as when he cites the Victorian sociologist, Henry Mayhew, who had worked out (and mapped) the geographical incidence of crime. It seems that while total criminality did not vary, the townsman was addicted merely to burglary, larceny, forgery, pocket-picking and shop-lifting, while the countryman specialized in crimes 'of the kind named unmentionable' (which incidentally included illegitimacy). And after all, says Mr Burke 'burglary and thieving' are fairly wholesome and quite natural activities.' Many of our towns, so he thought, were at least as old as our villages, for 'when [man] rose from savagery it was instinctive in him to gather with his fellows for mutual protection, for the exchange of knowledge and for the sharing of experience'. Such an opinion may pass muster in a book which, in point of fact, makes very entertaining light reading. But unfortunately it is the kind of opinion that is very widespread, and in particular our Planners, like Mr Burke, have never read their Gordon Childe. Build some houses, add a so-called 'trading-estate' (actually a congeries of small factories), 'decant' the 'over-spill' of some growing city into the houses and 'steer' some industrialists (or bribe them) into the factories : there is your recipe for a New Town. The habit of studying present-day cities in their functional aspects, and of examining the relationship between function and geographical situation has not yet spread from the geographers to the borough engineers, borough surveyors and county architects who form the corps d'élite of physical Planners ; still less of course do these experts probe with the archaeologist and the historian into the problem of the roots and origins of urban life. In the United States there is evidence of a wider vision, and if the young men and women reading philosophy, history and economics at Oxford together with their contemporaries reading mathematics and physics at Cambridge were even to flit through just the Syllabus and Maps of the Chicago course in Anthropology described by the Editor in the September number of *ANTIQUITY*, our future governing classes might be in a better position to resolve the antithesis between Plato and Karl Marx.

The most significant geographical element in the rise of civilization has always been accessibility : particularly accessibility by water. For this implies a limited and

* *The English Townsman*, by Thomas Burke. Batsford Books. 12s 6d net. *Water Transport Origins and Early Evolution*, by James Hornell. Cambridge University Press. 30s net. *Outline of English Architecture*, by A. H. Gardiner. B. T. Batsford Ltd. 12s 6d net. *English Church Monuments, 1510-1840*, by Katherine A. Esdaile. B. T. Batsford Ltd. 21s net. *Stained Glass in Somerset, 1250-1830*, by Christopher Woodforde. Oxford University Press, Geoffrey Cumberlege. 42s net. *A Millennium of Facts in the History of Horsham and Sussex, 947-1947*, by William Albery, Horsham.

ROOTS AND ORIGINS

canalized movement and approach (which can therefore be controlled), the limiting factor being the capacity to produce a 'sufficient ship'. Mr Hornell's *Water Transport, Origins and Early Evolution* provides an admirable and invaluable guide to man's conquest of waterways, for the author's professional life as Marine Biologist and Fishery Officer, especially in the Indian Ocean and the East brought an extraordinary range of primitive craft under his direct observation. He lets no detail of their construction escape him and he is, besides, the acknowledged authority on the coracles and currachs of the British Isles. He treats the subject analytically, under three major groups:—A, Floats, rafts and kindred craft (which include the Egyptian reed boat and the Chinese junk); B, Skin-boats, coracles, currachs, kayaks and their kin; C, Bark canoes, dug-outs and plank-built craft.

With Mesopotamia and its earliest cities in mind, it is natural to turn to the descriptions of craft used on the Euphrates and Tigris. Here are still to be seen the buoyed rafts of antiquity, and the *quffa* described by Herodotus, a skin-covered coracle of coiled basketry. At the present day the famous bitumen of Hit replaces the cover of hide, while Mr Hornell also describes a raft of reeds, tied tightly at either end and smeared with bitumen, such as was referred to by Strabo and noted by Layard on bas-reliefs. A fourth type is a large and deep boat of wicker-work on a skeleton of poles which is pitched within and without and seems for all the world the very ark that Noah was ordered to build. Such craft are said to run up to twenty tons burden. The question arises, from which of these river types (if any) did the Sumerians develop a sea-going vessel for their trade with Oman? We are familiar with the rôle of the Red Sea corridor: favoured by its accommodating seasonal winds, by its shape, size and orientation, it facilitated the voyages of the great sailing ships which linked Egypt with Punt. The Persian Gulf provides a closely analogous 'nursery' ante-room to the ocean, but as to the manner of its first use we have no evidence.

Every conceivable floating object appears to have provided the idea for some kind of boat. Even the wise men of Gotham who went to sea in a bowl were not the innovators we had supposed, nor we ourselves when in childhood we essayed the pond in a tub. It is in Eastern Bengal that the net-work of sluggish waterways across a soft mud can safely be navigated in a 'vessel' made by the village potter. As to the skin-boat with wicker-work frame, Mr Hornell suggests that it was possibly noticed that a skin with up-turned edges would float and to this an inner frame-work was added. But surely such a floating skin would be very unusual whereas a basket of reed or wicker-work might often be seen floating and bobbing upon the water. For basketry is a very ancient craft indeed. The 'ark' in which his mother hid the little Moses was a covered reed basket which she took the precaution to smear with pitch before trusting the baby to the water.

The world map with which this well-documented volume closes shows the present and recent distribution of the main types of primitive craft. It would be most interesting if Mr Hornell could set beside it a companion map showing the maximum extension of these types in antiquity as indicated by history and archaeology. The bark canoe in Asia, for example, is used no further west than the Ob, but surely the boats that (as related to King Alfred by Othere) the 'Queenes' carried on their backs when they crossed the Scandinavian watershed from the east to harry the Northmen, must have been of this type? (1)

¹ Here is an opportunity, with Mr Hornell as guide, for the National Maritime Museum to build up a genetic exhibit of boat models. At present it is centred too closely on the old-fashioned notion of 'relics' and 'specimens' to meet the needs of the serious student.

ANTIQUITY

To turn from *Water Transport* to the next three books under review is to enter another world. Boats have developed in response to urgent necessities of transport. Architecture, stained glass and church monuments, too, have their origins in human needs, the need for a home, the need for reassurance when confronted with death. But it is not the origins, but the end-products that these authors consider, and that solely as art forms. The two Batsford books, the *History of English Architecture*, and *English Church Monuments 1510-1840* are beautifully illustrated and eminently readable. In the latter, Mrs Esdaile definitely breaks new ground. Mr Christopher Woodforde's *Stained Glass in Somerset, 1250-1830*, with its magnificent plates, many in colour, is in a different category, a work of mature scholarship intended for the expert. All three writers are dealing with crafts, but the actual craftsman is strangely elusive. Elusive, that is to say until he steps up in the social scale and becomes the modern sculptor, architect or artist. Mrs Esdaile is wholeheartedly for power and place, and her book is urbanely and learnedly introduced by Mr Osbert Sitwell, apostle of the baroque. The reviewer, reflecting on these adornments of churches in relation to the masses who knew no other picture or picture book, cannot avoid the conclusion that the English Reformation emptied out the baby with the bath. A fifteenth century window at Banwell has an endearing representation of an actual baby being actually bathed in the great cauldron slung over the fire which served at other times to boil up the pease-porridge or seethe the meat. It illustrates the story of a pious mother who hurrying off to Mass quite forgot where she had left her infant; whereupon 'the water waxed hot, and then began to boil, to wallop and to roar.' But when she rushed back the baby was not boiled as she expected; he had not even changed colour, but was playing with the bubbles! What a loss for the village child when in place of pictured miracles such as this he had only 'heraldic glass' to ponder upon. The craftsmen, too, had to adapt their cartoons, patterns and traditional models to the new values. The long waved tresses and naïve loveliness of the Madonna had to be transferred to the effigy of some middle-aged Countess lying on her tomb, and copied half-a-dozen times over in her row of kneeling daughters. The Holy Babe was re-shaped to represent some sprig of nobility or royalty who died in infancy. Instead of the moving picture of the Entombment with a tortured Saviour, and a Mother's face twisted and ugly with grief, the child at church gazed at a sculptured group representing the death-bed of a young gentleman of family. Handsomely dressed and with composed though serious faces, the seated parents watch their son expiring while apparently in the pink of health, and with not a curl or a shirt-pleat awry. Cherubs, too, who had mysteriously existed with only a torso found themselves transformed into cupids with pretty rounded bottoms. Yet these little fellows might be interesting, such for example as the two leaning gravely intent from some solid marble clouds and trying to fit a too-small heavenly crown on to the immense peruke of a self-important gentleman who seems unaware of what is going on.

Sometimes, rather to Mrs Esdaile's disgust, the lower orders, too, purchased ornate monuments. But heredity will out. Unlike the exquisite little aristocrats who sprang from lords and ladies, the poor mommet whose father was 'in trade' is discernible even on its tomb as 'a really vulgar babe', though dead too young either to lisp or toddle. As it happens a noble viscount lies in sculptured state in the Church at Chipping Campden. His monument is not reproduced here, but no doubt the authoress has seen it. Did she recognize, beneath the coronet, a former plain Mr Baptist Hicks, the Elizabethan money-lender?

Mr William Albery who writes lovingly of Horsham brings us back to the townsman again, but inhabiting this time the small country town. He himself is appropriately

ROOTS AND ORIGINS

enough a master-saddler, for we have it on the highest authority that Horsham is simply and literally Horse-ham. Named in a Saxon Charter of A.D. 947 this ancient borough has the usual broken series of records—a fifteenth century Manor Roll, a seventeenth century manorial survey, the minutes of a Court Leet, enclosure awards and the like. But it was also an Assize Town, and housed the Sussex County Gaol, and so Mr Albery has a long tale to tell of English justice and injustice, crime and cruelty. Six chapters are devoted to the labourers' distress and violence in the 30's, and Horsham folk can reflect that these were their own great-grandfathers who were hanging young farm-hands (encouraged to do so by Lord Melbourne himself) made desperate by a wage level of 4s to 6s a week. Of course this did not only happen at Horsham, but history comes home to us if it is our home history. And Mr Albery's fellow townsmen have shown their appreciation of his long years of research by subscribing for the publication of these annals of a fragment of Sussex. Horsham had its roots and had its function. 'Tyme out of mynde,' declared the jury of burgesses in 1611, the yearly fair and the weekly market had been held and the tolls collected by the Corporation. Towns do not 'spring up' at random, nor can they be conjured into existence by decree. They are a natural growth as 'Man makes himself'.

Notes and News

CAVE-MAN IN FRANCE

The following note records the finding of what Mr Movius calls 'the most important human fossil to be exhumed in France during recent times'. We are allowed to reprint it from the *American Anthropologist* (Vol. 50, No. 2, April-June, 1948, 365-7) by courtesy of the Editor and of Mr Movius :—

During August 1947, the remains of a new and extremely important fossil man were discovered in Central France by Mlle. Germaine Henri-Martin at the Cave of Fontéchevade (Charente) (1). Previous digging at this site by various archaeologists had revealed the existence there of a series of horizons, the industries of which range from the Mousterian to the Magdalenian. Underlying the Mousterian layer a firm stalagmite floor had until recently discouraged further investigations.

This same thick stalagmite floor was traced to the slope in front of the cave, where Mlle. Henri-Martin has succeeded in penetrating it with significant results. The underlying deposits, which present a maximum thickness of some seven metres, are composed of red, sandy clay and earth; this stratum contains flint implements in direct association with a warm temperate fauna. According to the excavator, the implements have been examined by the Abbé H. Breuil, who considers them to be the same as those found by him in the lowest archaeological horizon at La Micoque, and known as the Tayacian (2).

The fauna is characterized by the following forms: *Rhinoceros (Dicerorhinus) merckii*, *Dama* sp., *Hyaena* sp., *Cuon*, *Testudo groeca* (3), etc. The forms represented, as well as their climatic implications, confirm a Third Interglacial dating for the deposit in question.

Mlle. Henri-Martin, whose systematic and meticulous excavation technique has been commented on by several competent observers, states that the objects from the basal horizon at the Cave of Fontéchevade occur for the most part in pockets, where bones and flints are normally associated. It was in one of these pockets at a depth of 2.60 metres below the surface of the ground and 70 centimetres below the base of the undisturbed stalagmite floor that the fossil human remains came to light.

The main find consists of an incomplete cranial vault, including the following articulated bones: part of the frontal, the two parietals, and part of the left temporal. Among the detached fragments found nearby, there is also a portion of an occipital, apparently of the same individual.

This partially complete brain-case, which was in an extremely fragile condition when first brought to light, had to be hardened before it could be disengaged from its matrix of red earth. Dr H. Vallois, the results of whose preliminary observations have just

¹ Henri-Martin (Mlle.), Germaine. 'L'Homme fossile Tayacien de la Grotte de Fontéchevade,' *Comptes Rendus des Séances de l'Académie des Sciences*, T. 225, pp. 766-7. Paris, October 27, 1947.

² Breuil (Abbé), H. 'Le Paléolithique ancien en Europe occidentale, et sa Chronologie', *Bull. Soc. Préhist. Française*, Vol. 29, No. 12, pp. 570-8, 1932; Henri-Martin (Mlle.), Germaine. 'Note préliminaire sur un Niveau Tayacien dans la Station préhistorique de Fontéchevade (Charente)', *Ibid*, Vol. 43, Nos. 5-6, pp. 179-182, 1946.

³ Henri-Martin (Mlle.), Germaine. 'Une Tortue fossile dans la vallée de Fontéchevade (Charente)', *Ibid*, Nos. 3-4, pp. 86-87, 1946.

NOTES AND NEWS

been published (4), states that it is a small-sized vault, and that it can have belonged only to an adult. It is of average length, but the breadth measurement is very low; it has the general ellipsoid form of a pronounced dolichocephal. The bones themselves are thick and robust, but not to a pronounced degree. Dr Vallois further states that, although the lower portion of the frontal is missing, the segment that remains does not lead one to suspect that the individual represented could have possessed brow-ridges of Neanderthaloid type.

The absence of the supra-orbital portion of the frontal bone of Fontéchevade No. 1 is in large measure compensated for by the fact that the fragmentary remains of a second cranium (Fontéchevade No. 11) include the glabella region and a small portion of the upper internal border of the left orbit. These latter remains were found in the same horizon and at a distance of approximately three metres from the sealed pocket that contained individual No. 1. According to Dr Vallois, the glabella region of Fontéchevade No. 11 is scarcely raised, while the upper border of the orbit presents a sharp, acute angle. Indeed there is nothing whatsoever that even remotely recalls the massive supra-orbital crest, so characteristic a feature of Neanderthal Man. On the other hand, with respect to the morphology of the partially complete frontal bone of individual No. 11, they conform absolutely with a fossil *Homo sapiens* type of man, in the opinion of Dr Vallois.

There can be no question concerning the fact that these finds were *in situ* when discovered by Mlle. Henri-Martin: they come from an undisturbed horizon sealed below a thick, unbroken and continuous layer of stalagmite that underlies the Mousterian level at this locality. Furthermore, the fauna demonstrates that these deposits were accumulated under conditions of the warm temperate climate of Third Interglacial times. And the archaeological material is definitely older than the Mousterian from a typological point of view. The essential point is, however, that this is the first time that Lower Palaeolithic fossil human bones have been discovered in France in a clearly dated stratum. Not only does this evidence further confirm the implications of the physical type represented by the even older (Second Interglacial) Swanscombe Man (and presumably Piltdown and Galley Hill as well), but it may be accepted as further proof that prior to the Würm 1 Glacial Stage, a race of men with definitely non-Neanderthaloid-type skull vaults was living in Western Europe. But, as Dr Vallois has commented, to date we do not possess any very satisfactory evidence concerning to what extent these pre-Neanderthal Europeans exhibited primitive characteristics, with the exception of the brain case. Certainly that they were of fundamentally modern type with respect to the latter feature can no longer be denied, even by the most profound sceptics.

HALLAM L. MOVIOUS, JR.

EXCAVATION OF A CHEVIOT HILL-FORT

The excavation of Hownam Rings, a hill-fort in Roxburghshire, was directed for the Society of Antiquaries of Scotland by Mrs C. M. Piggott, F.S.A. The site is 1000 feet up on the northern slopes of the Cheviots, and proved to be of four periods, within the Early Iron Age and the Roman period.

The earliest structure was a palisade trench containing post-holes packed round with stones and indicating that a fence of wooden posts each approximately 6 inches in diameter and probably laced together with some form of hurdling, had enclosed the

⁴ Vallois, H. V. 'Un Homme fossile Tayacien en Charente', *L'Anthropologie*, T. 51, Nos. 3-4, pp. 373-4. November 1947.

ANTIQUITY

south side of the hill. It had evidently been found necessary to replace this fence by another similar one a short time after its first construction. It was not possible to date this early phase but it is probable that it lies within the 1st century B.C.

The second phase was marked by the hill top being enclosed by a defensive stone wall some 12 feet across at the bottom and presumably standing to an original height of eight or nine feet at least. This fort was provided with two entrances, one on the north (not proved by excavation) and one on the south which was cleared in the excavation this year, when it was found to have been blocked with stones which included the lower stone of a quern of 1st century type. The closing of this entrance marked the beginning of the third phase of occupation, when the old wall defence was considered obsolete and was lowered in height and incorporated in a sloping rubble rampart, the material for which was partly obtained from a ditch. At the same time additional concentric lines of ramparts and ditches were added all round, and a new entrance made further down the slope and to the southwest of the wall fort entrance. It seems likely that this modification from single to multiple defences may have been made under the direction of political refugees from the south fleeing north before the Romans.

But these defences were short-lived. For we find the fourth phase, already established by the mid 3rd century A.D., was one of peaceful settlement, and circular stone-built huts were constructed partly over the now-obsolete rampart. A large quantity of coarse native pottery was found in the two huts excavated, and other associated finds included Roman sherds of the mid 3rd century A.D. and a blue glass bead and a fragment of a yellow and white glass armlet of approximately the same date.

It seems likely that this sequence is typical of many Border forts as yet unexcavated, but which show surface indications of a similar history.

This excavation served as a training centre in excavation technique for students under the Scottish Field School of Archaeology's scheme for practical instruction in field-work. At Hownam Rings, in fact, the entire labour force was provided by students, often working under very trying weather conditions.

A full report will appear in the *Proceedings of the Society of Antiquaries of Scotland*.

URN-FIELDS AND VITAL STATISTICS

It is the purpose of this brief note to direct attention to work recently carried out in Sweden on the determination of the age and sex of cremated human remains. The investigations have been carried out by Dr Nils-Gustaf Gejvall of Stockholm (1). The material comprised some two hundred cremation burials forming an urn-field cemetery of La Tène III Age in the parish of Horn in Västergötland. By careful and minute examination of every scrap of material, including the use of Röntgen photographs, and by the employment of biometric analysis, Dr Gejvall claims to be able to identify adult males, adult females and children and young persons and within these categories to assign individuals to their appropriate age-groups. How far his results can be accepted at their face value only an expert in these matters can say, but the objective manner in which Dr Gejvall has explained his methods and the fullness of the biometric tables appended to his report inspire confidence in the layman. This is enhanced by the knowledge that Dr Gejvall has tested his methods by reference to a hundred and one cremations of persons of known age and sex from two modern crematoria, and further, as

¹ 'Gravfältet på Kyrkbacken i Horns Socken, Västergötland II. Antropologisk Del', *Kungl. Vitt. Hist. och Antikv. Akad. Handl.*, del. 60:2. Stockholm, 1948. Cf. the same author's 'Bestämning av brända ben från forntida gravar', *Fornvännen*, 1947.

NOTES AND NEWS

regards the sexing of adults, by the confirmation given by such diagnostic-grave-goods (e.g. swords) as were found in the Horn cemetery.

The theoretical importance of this line of inquiry is evident, since it opens up the possibility of recovering vital statistics bearing on the age-composition and expectation of life of prehistoric populations, and on the size of individual communities, for the long and crucial periods during which urn-field burial prevailed over extensive parts of Europe. The practical implications are also important. In future the excavator can no longer content himself with emptying out the ashes and removing the odd bead, comb or brooch, or he may be guilty of pouring away data from which modern science can extract the vital statistics of pre- and proto-historic communities. Conversely, it may be suggested, we have here a most promising field for co-operative research by physical anthropologists and archaeologists. In the past too much energy was lavished on ethnic determinations and not nearly enough on the sociological data to be won from a study of human physical remains. This tendency has been corrected lately, particularly in connexion with collective burials in chambered tombs (2), and Dr Gejvall's work would seem very greatly to expand the possibilities by bringing within the range of determination peoples who burned their dead as well as those who interred them.

GRAHAME CLARK.

RECENT DISCOVERIES IN AZARBAIJAN

Twelve years ago Sir Aurel Stein carried out a small excavation at Hasanlu, close to the southwest corner of the Lake of Urmia, now called Rezaïyyeh. At that excavation he obtained material of the end of the second millennium, and this he published in his 'Ancient routes in western Iran'. His collection of material has been considerably augmented by the British excavations of the summer of 1948 at Geoy Tepe, close to Rezaïyyeh, an ancient site mentioned by Lehmann-Haupt in his 'Armenien, einst und jetzt'. The work, which was partly financed by the British School of Archaeology in Iraq, was very considerably aided by the kind assistance and co-operation of the Iranian authorities.

The top of the Tepe is about eighty feet above the spring of water close to its foot, but the level of virgin soil on which it is built may, of course, be a little higher than the level of the spring. The main shaft dug descended forty-four feet in the time granted for the work—six weeks—and at the lowest level reached produced red polished ware, the only shapes being straight-sided bowls. Above this level was one in which there were very large buildings, of which not very much could be traced in the shaft, which at that depth measured 22 by 24 feet in area. The pottery here was similar to the Al Ubaid ware of Iraq, though the shapes recovered are unusual. Seal impressions and minor objects of stone and metal were also found in this level. The stratum above contained grey polished ware, decorated with rather elaborate motifs in relief, which are placed just below the neck of the vessel. The spiral appears in these motifs. In this same stratum were found models of long-horned cattle, and bone-gaming pieces. So far as is known at present there are no parallels between this grey polished pottery and that of the Astrabad area, to the southeast of the Caspian Sea, except in the colour; but some other grey wares have been found at Tabriz, which may perhaps prove to be closer to the Shah Tepe fabrics. They have not yet been unpacked for study. The next stratum was much thicker than those below, and contained black polished wares of a homogeneous type. Bowls and jars were found, with rather elaborate handles and a curious type of decoration consisting of a horizontal line of flat circular depressions, about a centimetre across,

² e.g. Lanhill Long Barrow. See *Proc. Prehist. Soc.*, 1938, 131 ff.

ANTIQUITY

alternating with a diagonal incised line. The effect is oddly like the circles joined by a diagonal line which is fairly frequently found as decoration on Early Aegean pottery. Above this stratum was one virtually barren, and then one in which pottery of late Susa II type occurred, and polychrome ware. Still higher were fragments of the elaborately spouted tea-pots already known as coming from this area, and published in Stein's book mentioned above.

The polychrome ware was considered important, and several small shafts were dug in an effort to locate more examples. These were successful. The material, which has connexions with both Susa II ware and with the polychrome wares of Anatolia (Alishar III ware), is mostly dark on light, though one example of polychrome on black, in white and red paints, was also found. Several tombs were also found, dating from this period, and a considerable quantity of pottery, but only red polished and monochrome, and necklaces and bronzes, were recovered therefrom. A small smithy was discovered, and pieces of metal ores, including iron, may prove to yield interesting results after analysis. In one of the tombs a pot, possibly of iron, was found. Magnificent obsidian arrow heads were also found, some with the same shape as those found many years ago by De Morgan on the Caspian coast. The people of this type were dolichocephalic.

In strata later than that of the polychrome pottery there was found ware, usually polished, and red or black in colour, but without decoration. Shapes included beak-spouted vessels of tea-pot shape, as published by Sir Aurel Stein. The cemetery of this period also was discovered, but there was no opportunity to do more than open one of the tombs, a stone-built chamber with slightly corbelled walls, containing a considerable quantity of pottery—dishes, jars and cups, and bronzes and beads. It seems likely that the date of this material is of the later part of the second millennium. Still later in date is a mass of rather rougher pottery, usually red, or dark grey. A characteristic of this ware is roughly incised decoration, and another is the frequent appearance of elaborate handles, sometimes in the form of animals' heads, but also of more usual shape, adorned with knobs and thumb-pieces on the top, a type of handle common in Cyprus during the Bronze Age. This pottery is, maybe, of about the close of the second millennium.

The finds, which have only just arrived in England, will be put on view during the next few months, probably in Oxford. They are, almost in their entirety, in England, and we can be very grateful to the Iranian Department of Antiquities for having made so generous a gesture as to permit their export which, by keeping them together, facilitates to the highest degree the study of the early civilizations of Azarbaijan.

P. BURTON BROWN.

ARTHUR'S BATTLE OF BREGUOIN

One of the twelve battles said by Nennius, chapter 56, to have been won by King Arthur over the Saxons was *bellum in monte qui dicitur Agned*, according to most of the manuscripts; in spite of various efforts at identification Agned remains unknown. But one group, consisting of the Vatican Reg. 1946 MS, 11th century, and the Paris MS Lat. 11108, 12th century, which represent a recension of Nennius made in the year 944 (the 'Vatican Recension'), do not give Mons Agned at all. Instead they have a different battle, *bellum in monte qui nominatur Breguoin* (Paris MS *Breuoin*), . . . *quem nos cat Bregion appellamus*. This has been identified with Kirkby Thore, Brougham, Leintwardine, and other places; the first two are impossible, and the third only possible if we emend to Breguein and Breuein, as I have shown in *Modern Philology* XLIII (1945) pp. 52 ff. At that time I could propose no solution, and was inclined to think that Breguoin-Breuoin (the two are merely variant and equivalent Old Welsh spellings of the

NOTES AND NEWS

same name), and the *Bregomion* given by some MSS as a gloss on Agned, all represented a corruption of an original **Breguoinion*. But it now seems possible that Breguoin-Breuoin can be identified, and is therefore a real place. If so, it must be that the compiler of the Vatican recension substituted Breguoin or Breuoin for Agned; that he glossed it with *Cat Bregion*, 'The Battle of the Uplands', possibly another traditional battle which he considered to be the same; and that those late MSS which give *Bregomion* as a gloss on Mons Agned obtained it from a corrupt copy of the Vatican recension.

After the publication of the article referred to, Mr P. K. Johnstone kindly reminded me of the *kat gellawr Brewyn*, 'the battle of the cells (or "storehouses", or "chapels") of Brewyn', Book of Taliesin p. 61, ll. 16-17. This is given in the poem as one of the battles of Urien of Rheged, along with *kat yn ryt Alclut*, 'the battle in the ford of Dumbarton', and others. Now, Middle Welsh Brewyn might very well be spelt in Old Welsh as Breguoin or Breuoin (the *gu* is an Old Welsh spelling for *u=w*), and it seems most likely that they mean the same place; either that Arthur and Urien happened to fight a battle at one and the same site, or (which appears more probable) that a traditional battle of ancient legend was ascribed now to Arthur, now to Urien. In either case, since Urien was involved, the place was somewhere in the North.

Old Welsh Breguoin or Breuoin, Middle Welsh Brewyn, would most naturally represent a British **Brevēnium* (not Bravēnium, as I stated by an oversight in the above-mentioned article). No such name is known. But occasionally a Primitive Welsh *v*, from British or Latin *b* or *m*, became lost before the diphthong *ui* (older *ē*). Thus Latin *habēna* became Old Welsh **awuin*, whence first **awuin*, then **awyn*, and then the modern *awen*; or *Samuēl* became Old Welsh **Savuīl*, spelt *Samuīl* in the Old Welsh Genealogies (10th century), later **Sawuīl*, spelt *Sauuīl* in the Life of St. Cadog (end of the 11th century), whence Middle Welsh *Sawyl*. Applying this to Brewyn and Breguoin-Breuoin, it might very well represent a form with *-wui-* from Primitive Welsh **Brevuin*, and indeed the *-guoi-* and *-uoi-* particularly favour this. Whether *-vui-* could have become *-wui-* so early as A.D. 944 is not proven, the examples otherwise known to me belonging to the 12th and end of the 11th centuries; but there seems no good reason to doubt it. If so, however, the Vatican recension compiler's source is likely to have been a contemporary oral rather than an old written one; probably a recited poem.

Accepting this **Brewuin* from **Brevuin*, it would come from a British **Brebēnio-* or **Bremenio-*. The former is unknown, but the latter is surely the Romano-British Bremenium, High Rochester on the Border. Anyone who knows High Rochester will realize that the gloss 'The Battle of the Uplands' is very apt. As to the 'cells of Brewyn', might these not be the ruined foundations and cellars of the Roman fort? The site is a strategic one, and more than one battle may well have been fought there. Further, it would be very well suited to a fight by Urien against the Saxons; and those who believe Arthur was a champion of the North will find support for their view in this identification.

KENNETH JACKSON.

Reviews

NUBIAN TREASURE. By WALTER B. EMERY. *Methuen & Co. Ltd., London, 1948.*
x+72 pages, 10 text figures, 48 gravure plates. 10×6½ inches. Price 30 shillings.

This book is a well-illustrated account of the remarkable discovery, some eighteen years ago, of the tombs of the Nubian kings of the 5th and 6th centuries A.D. at Ballana and Qustul, a little to the north of the Egypt-Sudan boundary. At the time of this discovery Mr Emery was Director of the Archaeological Survey of Nubia, and the present volume is an abridgment of his official report of the excavation. 'It is', says the author in his preface, 'not in any way intended for the archaeologist, or even for the amateur in archaeological research, for it has been written entirely for the layman who may have an interest in ancient history and the materials of which it is made'. Mr Emery goes on to comment on the somewhat sensational publicity which the modern Press sometimes gives to discoveries in the archaeological field—and from which his own discoveries were not exempt—but he considers that this reflects the growing interest of the public in history and the lessons it can teach, and that some attempt should be made to satisfy this interest. He continues: 'Archaeology and history remain, to a large extent, the interest of a few, and I feel that the science has little right to existence unless its fruits are made easily accessible to the layman'.

With such sentiments there will be much more general agreement among archaeologists than might have been possible some years ago. We are all of us, nowadays, being made increasingly aware of basic economic facts, and, to bring the matter to its lowest level, the archaeologist cannot reasonably expect to be fed and clothed by the community unless its more intelligent members are convinced they are receiving something of value in return. Yet, however widely the archaeologist may wish to disseminate the results of his work, he can, in practice, make them accessible only to the layman who is prepared to read, and even to buy, what the libraries call 'non-fictional' books. The publishers of *Nubian Treasure*, at any rate, have envisaged a layman sufficiently interested in history and archaeology to be prepared to pay thirty shillings for a book which can be read at a sitting.

Now it is to be doubted if the correct approach to a layman of this kind is to impress upon him at the outset that he is being offered something beneath the notice of the serious archaeologist, even though the disclaimer may be intended to disarm professional criticism. The reader may somewhat resentfully feel that he is being written down to and that his intelligence is being underestimated. A method which would more nearly approach the ideal would be to produce books which would deserve the respect of layman and professional alike. This is by no means impossible. The layman, it is true, cannot be expected to buy and read detailed excavation reports, nor can there be any relaxation of professional standards in regard to their preparation. There is, however, nothing esoteric about them; they are unreadable as literature because they are primarily and essentially works of reference intended for all time. They neither excite nor satisfy public interest because they do not usually appear for years—in extreme cases half a lifetime—after the excavations they put on record. A preliminary report is a different matter. If such a report, giving a first-hand, authoritative, general

REVIEWS

account of the discoveries made could be published promptly in book form, instead of (as often at present) in a technical or museum journal, it would be exactly the sort of book the present reviewer has in mind. It would satisfy, without condescension, public interest at a time when that interest was aroused, and it would meet the immediate needs of most archaeologists. For those of the latter who were concerned with some branch of archaeology other than that dealt with, but who wished to keep abreast of progress in fields outside their own, the book would suffice, while the interested specialist could await with dignity the publication of the detailed and final report, which would find its resting place on his library shelves. Further, such a book would probably show a profit to its publisher.

Except that, through no fault of the author, it appears somewhat late, *Nubian Treasure* is very nearly a book of this kind, but it falls short of complete success because Mr Emery could apparently not quite decide to which section of the public he should address himself. The title might, perhaps falsely, suggest that he cast an eye in the direction of the readers of the more sensational newspapers, but it is evident that, once immersed in his task, Mr Emery found himself writing for that more limited, literate public which can read with interest and profit not only an account of the excitement of discovery, but also the outline of the history of Nubia which the book contains.

Despite the criticism which has been made, *Nubian Treasure* can be recommended to all those, laymen or otherwise, who have not had an opportunity of reading the official report of the discoveries with which it deals. The plates, a lavish and impressive series, have been thoughtfully arranged so that a description of each object is printed on the page opposite to the photograph which illustrates it. When so much has been given it might seem ungracious to suggest that an air photograph of the ancient irrigation channels mentioned on p. 42 might usefully have been included. F. ADDISON.

ARCHAEOLOGY: A magazine dealing with the antiquity of the world. *Published quarterly by the Archaeological Institute of America. Printed by T. O. Metcalf Company, Boston, Mass. Subscription \$6 per volume in the U.S.A. Foreign postage \$0.50 additional. Single number \$1.50.*

The first number of this new journal has a celt from Asine as its frontispiece and is then introduced by Dr Sterling Dow in a jocular foreword which leaves the reader in some doubt as to the public to which the journal is appealing. *Archaeology*, he says, 'has a single distinguished ancestor (*Art and Archaeology*) a slightly dubious but popular parent (the *Newsletter*) and two second cousins. One of these cousins, a German, has died. The other is living quietly in England' (not named but presumably ANTIQUITY). After alluding to *Archaeology's* respectable but distant British cousin, Dr Dow states that the new journal has no competitor, and makes what seems to me a most unjustified claim that 'no other technical magazine exists which similarly draws upon all the world's archaeology' (unless I have misunderstood his words, which is very possible).

The journal as a whole, however, is much better than its foreword might lead one to suppose. It opens with a pleasant Summary by T. C. Lethbridge of the spectacular Saxon burial at Sutton Hoo (published in ANTIQUITY, March 1940), but why does he think the hanging bowl may have been made in Ireland? *Pots' Progress*, by Miss Talcott, describes how the American excavators of the Athenian agora handle their material and tabulate it. All who like myself have been privileged to consult the agora materials under the courteous guidance of Miss Talcott can testify to the ease with which this 'material' can be studied. Glanville Downey describes an interesting Byzantine

ANTIQUITY

exhibition at Baltimore. Particularly exciting are a silver drinking vessel dated about A.D. 1000 belonging to Mr Louis Clarke, and a liturgical treasure of the 6th century found near Hamath. Downey also summarizes the recent progress of Byzantine studies in the U.S.A. The next two items are so short that they are just tantalizing. I hope Mr Healey will write a serious article on the Lacandonas and Bonampak. I should also like to hear more about the neo-classical architecture of Ovid, New York, though the subject seems to belong to art rather than to archaeology.

'Ostia' on the Tiber by H. T. Rowell is an excellent summary of the excavations on that site and their historical significance. The most exciting building is a unique 4th century basilica with a baptistery attached, a queer plan and possible associations with St. Augustine and his mother.

Professor Lehmann gives a careful but depressing account of the effect of the war on the antiquities of Samothrace, Mrs Hawkes summarizes briefly recent discoveries in Britain and T. H. Low discusses 'Who uses the archaeological material in American Museums?'

The number ends with some interesting news. Dr G. G. MacCurdy deserves a better obituary but perhaps will receive a further one later.

The second number of *Archaeology* bears on its cover a representation of the clay worker from the Metropolitan Museum, and opens with a series of photographs and captions by R. P. Schaedel on the astonishing monolithic sculptures of the Southern Andes. W. F. Grimes gives a good account of an extremely interesting Celtic temple which he excavated at the London airport, situated in an earthwork which has been known since Stukeley's time.

A. E. Raubitschek summarizes the archaeological evidences of ostracism in ancient Athens; five-sixths of the ostraka come from the American excavations on the Agora. After this comes a fascinating account of 'Castles in Zin' the district of Southern Palestine, now called the Negeb, by H. D. Colt, with some beautiful photographs (Fig. 6 is a study in tones). It includes items such as the story of Theodulus, the elaborate water system of the Negeb in early Christian times, and a very good basilica at Sbeita. I should like to hear a lot more about Zin. 'Cluny' shows wooden models of the third church which deserve an article in some later number. J. P. Harland writes a clear and attractive account of 'Life in a Minyan village', but I must protest that he is too clear, and suspect that some of his dogmatism is due to compression. Thus 'the Minyans spoke a dialect of Greek akin to Aeolic which we may call Arkadian' and 'metal—at first copper and then bronze, an alloy of copper and tin—appears to have come into use about 3000 B.C. a little before that in Egypt, and possibly a little later on the mainland of Greece'. I suspect Dr Harland would have qualified such statements more if he had been writing for the *A.J.A.* Nevertheless the general picture is good, and I like his account of the megaron.

Nine celebrated Romans await identification by the reader.

Then follows 'the Man about the Bones' by Mr G. Woodbury, a piece of entertaining journalese. There is no reason why a man should not make fun of his own profession, and I enjoyed the story of Big Charlie, but if Mr Woodbury has guessed correctly the object of this journal, then all the other authors of articles are wrong.

'Snake Dances' by Miss Lawler, is a series of photographs with suggestive captions.

The number ends with some brief notices of recent books.

A pleasant feature of the journal for an English reader is the welcome afforded to British news and articles, and I hope that later numbers will extend this courtesy further to news from 'East of Suez'.

R. W. HUTCHINSON.

REVIEWS

THE ATLANTIC ISLANDS: The Faeroe Life and Scene. By KENNETH WILLIAMSON.
Published by Collins, London, 1948. pp. 360, 35 photos, 4 maps.

This must be perhaps the only, certainly the most complete, modern account of the Faroes—that scarcely known group of islands that lies nearly midway between the Shetlands and Iceland. Studied during the enforced leisure of garrison duty during the war, there can be few details about the islands, their inhabitants and their natural history that the author has not here set before us, both in the text and in the appendices which are devoted to lists of mammals, birds, bibliography and glossary. The book is not in any sense archaeological, yet it contains much to interest the student of the past because it records living conditions of human life that have left only traces behind in other European countries. Of special interest are the descriptions of agricultural implements and methods, land tenure, and particularly the harvesting, drying and milling of corn. Customs and folk-lore are also of interest, especially for comparison with those of regions like the Hebrides where Scandinavian elements have been superimposed upon Celtic and earlier strata of the population.

The description of the method of drying the green ears of six-rowed barley (the only local corn) in kilns is important, not only for comparison with the kilns of the Shetlands, Orkneys and Hebrides, but because it will help us to understand the working of the not altogether dissimilar kilns that were used in Roman Britain. The primitive water-mills are also of great interest and are well-described and illustrated. The author regards them as belonging to a northern group distinguishable (along with other Scandinavian mills) from a southern or Mediterranean group, and suggests that the Vikings may have derived them from the Near East rather than from Ireland. But as the Irish mills seem to have belonged to the southern group equally with those of the Near East, this suggestion will not explain the differences of details that distinguish the two groups.

The book is very well written and abounds in delightful word-pictures, though one could wish that some of the descriptions of apparatus, etc., had been clearer. The numerous photographs, many of them from the author's camera, are excellent and succeed in showing very clearly what they are intended to show. E. CECIL CURWEN.

HIMMERLANDS OLDTIDSMINDER. By TH. RAMSKOU. *Published by Jul. Gjellerups Forlag, Copenhagen, 1947. 96 pp. 37 photos, and sketch-map. Price, 5 kr. 75.*

This attractive little book on the prehistoric antiquities of Himmerland in Denmark is written for the benefit of the intelligent Danish public. Himmerland is a district in eastern Denmark lying south of the Limfjord, once virtually an island, and still rather off the beaten track, and it contains a rich collection of prehistoric remains of all kinds, including the well-known Mesolithic sites of Gudenaa and Ertebølle. The various classes of antiquities—megalithic remains, barrows, house-sites, lynchets, etc.—are well described, and the series is illustrated with 37 full-page photographs which are attractive, informative and well-reproduced. This means that almost every second page in the book is occupied by a good photograph, which is a remarkable achievement and an example to be followed. The author and publisher are to be congratulated.

E. CECIL CURWEN.

AENEAS PONTIFEX. By H. J. ROSE. *Virgilian Essays, No. 2. pp. 28. London, Phoenix Press, 1948. 1s 6d.*

It is not altogether easy to see the reason for the publication of this essay. Its purpose is to examine the question whether Virgil's Aeneas is, as Vettius in Macrobius's *Saturnalia* says, represented as a Pontiff, whether in fact he in any way reflects Augustus

ANTIQUITY

as head of the Roman State religion. The conclusion is of course negative. There is really nothing in the *Aeneid* to show that Virgil meant to represent Aeneas as a Pontiff, and Professor Rose to his credit does not find anything, though he goes to great lengths sometimes in trying to detect some faint shade of pontificality in the hero's conduct on various occasions. The result is not particularly helpful to our understanding of the *Aeneid*: but in the course of his 'rambling discourse', as he himself calls it, the author produces from the vast stores of his learning in these matters a great deal of curious information about the Pontifices and ancient religion in general. For the sake of this the essay is worth reading.

A.H.A.

PRINCIPLES OF ANTHROPOLOGY. By ELIOT- DINSMORE CHAPPLE and CARLETON STEVENS COON. London, Jonathan Cape, 1947, pp. 718.

The publication of an English edition of Chapple and Coon should save this country quite a lot of dollars, since many readers ought to buy it and these should include archaeologists. For those who would decipher the archaeological record are compelled constantly to have recourse to the ethnographic record in their efforts to bring to life again the illiterate societies of the past. For their needs these *Principles* are particularly well-adapted owing to the prominence given by the authors to technology and 'material culture' in general.

One of the authors is primarily a psychologist while the other is best known as a physical anthropologist. But though the book opens with a concise but lucid account of the physiology of the brain and nervous system followed by a chapter on psychology and 'conditioning', it wastes no space on anthropometry, racial psychology or 'Blut-und-Boden'. Instead we have 200 admirable pages on environment and technology. The importance of the non-human environment is adequately estimated and the main general types satisfactorily characterized in chapter 5. But alone of all animals 'man can survive in all climates. The reason is that he alters his environment to suit his physiology instead of altering his physiology to suit his environment, as is the case with animals who thus become differentiated into numerous species . . . We know that this human versatility . . . was acquired by people practising the simplest known combination of techniques'—a conclusion derived from ethnography that is confirmed by archaeology.

So the authors proceed to a condensed classification and description of technologies. Beginning, like archaeologists, with cutting tools—chipped stone, polished stone, metal—they show how improvements in these are correlated with advances in house-building, body-covering, the food-quest and other economic activities. I have seldom seen the human significance of those favourite archaeological fossils, axes of polished stone and of metal, so well set forth with a commendable economy of words. The ensuing section on house-building is illustrated by a very full description of the construction of a round, pole-walled house among the Ethiopian Galla which gives all the significant details that are too often omitted by 'social' anthropologists (the exact methods of measurement for instance). Though these Ethiopian houses are only 18 feet in diameter, the account can be read with profit by the excavators of the round houses so common in the British Isles.

There follows an account of the techniques of processing food and other raw materials that is valuable not only for its good description of methods of conservation, fermentation, but for its insistence on the importance of preservation itself. 'Food-gatherers' acquainted with effective methods for preserving food may be better placed for the accumulation of a social surplus than 'food-producers' lacking that knowledge. The chapter on transport (which follows the discussion of methods of securing food) is to be

REVIEWS

welcomed for the same reason. Incidentally it points out that sleds are more serviceable than wheeled vehicles in damp or rocky environments. The articles on 'Horses and Battle-axes' in *ANTIQUITY*, xv, would have been improved by a perusal of this section. If it be fantastic to imagine Battle-axe folk careering through the swamps and forests of northern Europe in horse-drawn chariots, their steeds may have drawn sleds as they do in the mountains of Norway today over narrow stony tracks impracticable for carts.

Throughout their exposition the authors have been at pains to show to what extent the several processes and techniques described entail the co-operation of individuals or groups. For the aim of their 'operational method', explained in the first chapter, is to formulate as descriptive 'laws' functional relations between variables and so build up a science of human behaviour. Accordingly the description of productive techniques and processes leads inevitably to a discussion of division of labour. Here a very useful distinction is introduced between full-time and part-time specialization, both terms being accurately defined. Division of labour of course involves 'trade', the principal forms of which are summarized in the same chapter. The authors are then enabled to define eight stages of 'technological complexity' ranging from 'Subsistence—no specialization and less than 10 per cent trade' to 'Commercial—all or nearly all persons are full-time specialists, trade approximates (to) or exceeds 90 per cent'.

In part III, 'the Development of Institutions' is nominally built up on the basis of the technological classification reached in part II, but in practice there seems to be a partial relapse from what White calls the 'culturological' approach to the psychological approach, so congenial to the individualism of contemporary America. This appearance is enhanced by, if not due to, the technical terms—'set', 'interaction rate', 'originates to', . . . defined in chapter 12 and employed in the rest of the book. 'A set is an aggregate of relations of such a nature that every individual is a member of one of three classes within it as follows: (A) a class of individuals who only originate; (B) an intermediate class who respond to the origins of A and originate to members of C; and (C) a terminal class of individuals who only respond and who do so to the origins of members of both A and B'. After such a definition, it is difficult to avoid the impression that leadership is due to mysterious psychological properties of individuals. But in fact an effort is made, most successfully in the case of economic institutions, to establish correlations between the phenomena of leadership and the previously defined technological classes.

The last two Parts are entitled 'Symbols and Human Relations' and deal with Rites (divided into Rites of Passage and Rites of Intensification), the supernatural world, language, art, games and warfare, money, law, and science. It would be impertinent for an archaeologist to attempt an appreciation, still less a criticism, of these sociological chapters. It might even be out of place in an archaeological journal. For the purposes of the latter it should suffice to mention a few points that seem specially relevant.

Magic is, following Tozzer, defined as the technique of religion. Religion derives from rites the function of which is to restore equilibrium in individuals or in the groups at times of 'crisis'. 'Crisis' means a time of great change in the interaction rates of the individual or the whole group, for instance puberty and harvest, which upsets the equilibrium of those affected. In the one case the disturbance 'is countered by a series of techniques (rites of passage) requiring the interaction of the disturbed individuals in specific and habitual ways'. In the other case 'the techniques (rites of intensification) used enable the individuals affected by the changes to build up the new interaction rates needed to restore their equilibrium'. The complexity of the ritual techniques is shown to be correlated with the complexity of practical techniques previously defined.

ANTIQUITY

Incidentally we read how among the Aztecs 'farmers were buried since they worshipped a god associated with rain and the earth; the worshippers of most of the other gods (i.e. traders, craftsmen, warriors) were cremated'. Of course the Aztecs were a fairly complex and articulated society, but the passage is commended to those prehistorians who deduce conquests or invasions from the appearance of cremations in an inhumation cemetery or a general change over from one rite to another. At the same time it would have been relevant to the objects of the book to examine the relative wealth of the grave-goods in the several kinds of burial and the rules of inheritance among the different classes.

In the chapter on language the authors emphasize the 'importance of language in facilitating the execution of techniques; unless you know the names and uses of the various parts of a sailing vessel, you will be of little help in a storm. And it is these techniques, especially in trade, manufacturing and war, which tend to bring people together'. The authors go on to discuss rather summarily, the correlation between different types of language and cultural complexity, and the social significance of language differences within a society. 'Thus, the English are conditioned to these differences through their class system by which the Public School symbolizes origins and the Cockney the terminator class in the political system'. Having revealed language as a means of communication required in the first instance for the co-ordination of co-operative technical activity, the authors fail to proceed to consider its implications for the social creation of a rational 'world' out of a chaos of discrete sensations. To some extent this failure limits their account of science in chapter 19. It is none the less very competent. In particular the concrete account of methods of measurement used among illiterate societies today will help the archaeologist to appreciate the science of his preliterate ancestors.

In conclusion one should insist that for Chapple and Coon Anthropology is not concerned only with contemporary savages and illiterate tribes. Their surveys range from the Andamanese to the English, from Totemism to Christianity from subsistence to commercial economics. And, like archaeologists, they seek to draw historical conclusions from their comprehensive survey. 'The history of mankind throughout the world and throughout the time of which we have knowledge has followed a linear progression in two respects; (1) the complexity of human technology has increased so that men have been able to utilize more and more the resources of the terrestrial environments; (2) human relations and hence human institutions have also increased in complexity as a function of the increase in complexity of technology'. These are deductions from comparative ethnography, but they are confirmed by archaeology, which has the advantage of observing directly the time dimension that ethnography can only infer. V.G.C.

Antiquity

VOL. XXIII No. 90

JUNE 1949

Editorial Notes

THESE Notes are shorter than usual not from lack of copy but for the opposite reason—to allow more room for articles and reviews. We hope to return to the normal three pages in the next number. Making up each number of *ANTIQUITY* is always a problem of compression. We take this opportunity of apologizing to reviewers, authors and publishers for the delay in printing reviews; the only remedy is a larger circulation and the increase in size which that will make possible. A publicity campaign to effect this is now in progress, and we hope that any subscribers who may receive the leaflets will forgive us and remember that this is inevitable when one is distributing leaflets on a large scale to members of archaeological societies. We wish also to thank those societies who have helped by sending us lists of their members. It was upon such lists that *ANTIQUITY* was originally founded; the time has now come to bring *ANTIQUITY* to the notice of a new generation.

❧

The present number contains a new feature—‘Important New Books’. To borrow the words of a contemporary, the inclusion of a book in this list does not preclude its subsequent review. But we are sure that readers will like to know at once when such books are published. We shall include in this section articles that give authoritative accounts of important new discoveries and excavations. For the progress of knowledge is registered much more in the periodical journals and bulletins of learned societies than in books; and those who wish to keep abreast must consult them. The current numbers of such journals should be displayed on a table in the rooms of learned societies, but often they are not.

❧

This new feature was suggested by one of our most valued readers. We have often profited by such suggestions, and we invite our readers to write and tell us what they like, and also what they do not like, in *ANTIQUITY*. At the same time we are bound to say that we cannot always reply to such letters, for our correspondence is extremely heavy and we have no secretarial help. But we read all such letters and value them highly; they are a great help in every way. We hope that our readers have no grounds for complaint, either now or in the recent past; but if they have, we hope that they will also write and tell us, so that we may do our best to put matters right.

'Buried Landscapes' in Southern Italy

by JOHN BRADFORD

IT is widely known that war-time air photography has led to the discovery of many new archaeological sites of importance in Mediterranean lands. Many hundreds of tumuli have been added to the list, at such famous Etruscan cemeteries as Cerveteri and Tarquinia (1), and complete systems of Roman land-partition by Centuriation have been identified round the *coloniae* of Iader and Salonae, on the shores of Dalmatia (2). But by far the most notable discoveries of all are those on the Foggia Plain, in the Province of Apulia, in Southeast Italy. Great numbers of Prehistoric, Roman, and Medieval sites are being identified, and some preliminary results have already been published in *ANTIQUITY* ('Siticulosa Apulia', December 1946). Select examples were exhibited at the Classical Conference at Oxford and at the British Association Meeting, in 1948, and again for several months this year, in the Ashmolean Museum. These were chosen from a number which it was fortunately possible to acquire for the University of Oxford, now housed at the Pitt Rivers Museum, where they are being studied in detail. This collection was based on vertical (3) photographs taken by the Royal Air Force, and oblique photographs taken by Major Williams-Hunt and myself (which were the first to reveal this dense concentration of sites, spread more thickly on the ground than almost anywhere else in Europe). This heavy concentration is of much more than local importance. During the last few years I have examined many thousands of air photographs of Southern and Central Europe taken at various seasons, in the course of my research. While these provide much interesting data and give us, as it were, an illustrated 'Domesday' survey of Europe in the middle of the 20th century (of capital value to Anthropology), in no other area has there as yet been anything approaching the quantity of crop-marks, grass-marks, soil-marks and earthworks which have come to light in Apulia. There are various reasons for this and a detailed account must await a later report. For our present purposes, it will be enough to single out one or two areas, for comparison. In the plain of Lombardy, the patchwork of little fields bearing a great mixture of crops has paradoxically made crop-marks very rare, and much the same is true of the valley of the Rhône and of large areas of Southern France. In Central Europe, the survival of a strip-field landscape has had the same effect, for the planted, ploughed, or fallow strips are so mixed that it is impossible to find a good-sized area with a single crop. In the Danube Valley, where one would hope for Neolithic sites complementary to those in Apulia, or traces of a Roman centuriated landscape, the chances have been minimized by the re-distribution of small-holdings to peasants (in the last century) in narrow strips aligned within a graticule of roads which is equivalent to modern centuriation, and irons out earlier traces. In other areas of potential importance, as for instance in the coastal belt behind Bari or Naples, almost continuous gardens of

¹ 'Etruria from the Air', *ANTIQUITY*, June 1947.

² 'A Technique for the study of Centuriation', *ANTIQUITY*, December 1947.

³ The present illustrations are reproduced by kind permission of the Air Ministry, and are Crown Copyright. They were taken from heights between 10,000 and 25,000 feet, towards the end of May and early in June, and have been enlarged.

'BURIED LANDSCAPES' IN SOUTHERN ITALY

olives and vines severely restrict the possibilities of discovery from the air. However, even these are not entirely unrewarding.

But the reasons for the pre-eminent suitability of the rolling expanse of the Tavoliere di Puglia are quite straightforward and may well be met with again. They have been summarized in an earlier article, and it will suffice to recall the main factors. Saving its coastal marshes, this part of 'Siticulosa Apulia' has always retained the parched character it had of old. Throughout the Middle Ages, and indeed until comparatively recent years, very large areas were given over to pasture and untouched by cultivation, so that some big stretches of grassland have not been ploughed since the Roman period. The modern field-pattern that has grown up since the national state of the 19th century set about restoring agriculture to the region is made up of very large, open, arable fields. The conditions are ideal for crop- and vegetation-marks of all kinds, and earthworks have been allowed to fossilize in peace.

The topographical character of the region can be clearly seen from the scale-model, made and coloured by my wife and myself, shown on PLATE I (4). There is a natural unity about it, which favours our researches, although in Neolithic and Roman times it was economically linked with, and continued by, the coastal belt and inland uplands, to the south of it. The table-like plain of the Tavoliere is framed between the Apennines and the massive Gargano Promontory, and its natural boundaries to north and south are the Rivers Fortore and Ofanto, where the plain dwindles away. Let the reader imagine the setting: a buff and pale green landscape under a Mediterranean sky, shimmering in the intense clear sunlight, with belts of dark green round the huddled white towns; while the eye travels gratefully to the blue shady mountains which ring the plain. The sea is at hand and we look toward it, conscious that the opposite shores are near, and feel drawn in their direction rather than inland and up the Peninsula. Today, as in the past, this feeling reflects a real orientation in outlook and contact. The setting then is exhilarating and even harsh, not restful; for it is essential to capture the quality of the living landscape, before we consider those predecessors which are masked beneath it. The most important structural features, which have exerted a continual effect, stand out clearly in the model, which itself reproduces the nature of the terrain more effectively than does a map. The original is made in plaster of paris, working from a contoured and layered map, and has a horizontal scale of 1:250,000 (or 1 inch to 4 miles) and a vertical scale of 1 inch to c.1700 feet, carefully determined. The whole covers a land and sea area of some 62 by 72 miles, and includes the Tremiti Islands, on which are Neolithic sites. From the point of view of Human Geography, the first low undulating slopes of the Apennines belong to the oval plain of the Tavoliere proper. Certain features have special significance, which have been brought out in the model, notably the 100 metre contour-line (an escarpment well defined on the ground) which often marks the limit of the Roman field-systems laid out by centuriation. Neolithic homestead and village sites, however, are found all over the plain and right down to the edge of the three lagoons, Lesina, Varano and Salso, with their marshes and dunes.

Many of the photographs show traces of successive periods, as a palimpsest more complex than even those to which the late Major Allen accustomed us in his discoveries in the Oxford region. When the Apulian photographs are seen, it soon becomes apparent that one cannot take an exclusive interest in the sites of any one period or kind. Such specialization would be out of place in the cavalcade of cultures thus disclosed. We are

⁴ We wish to thank Mr H. N. Newton and Miss O. Godwin, of the Ashmolean Museum, for their skill in photographing the model from different view-points.

ANTIQUITY

watching; in effect, the stages in growth of a European peasantry, from its formation and over the last 4000 years. Anyone who knows Apulia well, or a similar peasantry, will also know how interbred are the present and the past, and that this continuity is not an abstraction. We need therefore an element of Anthropology in our Archaeology, here especially; and this means to say that our survey of Humanity from the air must also be Humanism from the air. The essential business of plotting the sites, measuring them, and comparing them must go forward, but we should always remember that these are the 'rough workings' as we move towards our human equations. To compare circles with circles, and squares with squares, is the elementary stage, though an indispensable foundation of fact. The human landscape is an organic thing continuously evolving through phases of growth and decay. In Apulia, the Neolithic and Roman pattern of settlement, so clearly revealed to us from the air, has vanished below the ground, but life and the same peasant stock continued. One could hardly wish for a better illustration of that idea of a sequence of 'Buried Landscapes', which Dr G. E. Daniel propounded in a recent broadcast.

It would not, in any case, be surprising that the largest area of plain in Italy south of the plain of Lombardy should be able to show many traces of settlement. Nevertheless there has often been a tendency to treat it as a remote backwater, an appendage to Taranto, or Naples and the Campanian plain. But the recent discoveries from the air strongly suggest that it deserves far more attention than it has yet received. Moreover these discoveries have a more than local application. The Prehistoric, Roman, and Medieval sites, present in abundance, have each in their several ways contributions of far wider significance to make to the archaeology and history of Europe as a whole, and these are dealt with below in turn. The necessity of following-up the photographic interpretation with field-work on the ground is well recognized, since the task of surveying and test-excavation is the necessary sequel, and complement, to all discoveries made by air-photography, and is accepted as axiomatic in this country. A failure to take this necessary and logical step would be a grave omission. Fortunately, however, an archaeological reconnaissance has already been approved in principle by the Italian authorities. Grants towards the cost have generously been voted by the Society of Antiquaries, the Prehistoric Society, the Craven Committee and other bodies, and, at the time of writing, I hope to begin a programme of field-work and test-excavation during the present summer under the auspices of the Apulia Committee set up through the Society of Antiquaries, and with the assistance of the British School at Rome.

NEOLITHIC COMMUNITIES

First we may summarize the conclusions set out in my earlier article. Over 200 (5) sites of 'village' and 'homestead' character have been found from crop-marks, some measuring up to 500 by 800 yards overall. This is one of the densest concentrations of prehistoric settlements yet known in Europe, lying as they do within an oval area of some 30 by 55 miles. These sites are all of cognate kind, and not to be confused

⁵ Probably this number would be doubled if further systematic air-photography was undertaken for short spells in early summer, over several years. We are reminded of the thick concentration of Neolithic settlement-mounds in Bulgaria, described by James H. Gaul in the *American School of Prehistoric Research Bulletin*, no. 16, 1948, p. 79. The 'vast' number of 220 have been recorded in Bulgaria, but this no longer seems so extraordinary in view of the quantity of sites on the Foggia plain. The great majority of those in Bulgaria are merely recorded, from ground observation, and thus there are very few plans. Gaul estimates a population of 5000-10,000, at any one time, in late Neolithic Bulgaria.

'BURIED LANDSCAPES' IN SOUTHERN ITALY

with any others seen in Apulia. They are all variations on a circular plan and are surrounded by broad ditches, from one to eight in number. Within them lie small ditched enclosures, of circular or penannular form, which are presumably 'compounds' containing dwellings, somewhat in the style of simple societies based on mixed farming found in parts of Africa today. The largest of these measures about 150 feet across, and the smallest about 40 feet. The mean overall diameter of the great majority is between 40 and 70 feet (including the ditch which is usually some 10 feet wide or more). In the 'homesteads' there may be no more than a handful of these, though in a large village 100 or even more can be identified. The diameter of the enclosed area is too large for the ditches to have played any part in the structure of dwellings within; and one may reasonably infer that they were intended to secure privacy, and perhaps also to keep out stray cattle or to act as windbreaks. Sometimes the circle is completed, in others it may have been rounded off with a portable barrier of hurdles or the like, which has left no trace on the photographs. It will be observed that some 'compounds' are much larger and more imposing than others. Whether this is a difference in status, in function, or merely in the number of persons living together, it is impossible to decide without the complete excavation of several of various sizes. But we ought eventually to be able to achieve some finality about the answer to this question. Within a given site, the opening appears almost always on the same side of the compounds. Again, at present it cannot be said whether this orientation is related to the position of the main entrance to the site, or if it was decided by custom, or even by the prevailing wind, or else lay conveniently in the direction of the principal pastures or tillage. Many other significant points can be collated directly from the photographs, but, to avoid repetition, it can be said that in the last two years further study has not made any change necessary in the details given in my earlier review, to which this forms a sequel. However there has been one important development which bears directly on our survey. This was the admirably clear and comprehensive classification of the pottery of Neolithic Apulia, by R. B. K. Stevenson (6), which goes a long way towards sorting out the successive cultural phases, and indeed, as far as is possible without further stratigraphical evidence. He suggests that ditch-enclosed settlements may make their first appearance alongside the fine wares arriving in his Phase 2 and developing in his Phase 3. General parallels for these wares are not lacking eastwards across the Adriatic, but it must be confessed that the exact area of origin proves somewhat elusive. His conclusions accord with the approximate date, rather before 2300 B.C., by which I suggested that the first of these ditched settlements had appeared on the Tavoliere. But if this is as yet vague, their end is still more so. Indeed, the degree to which we may think of them as continuing into the early stages of the Bronze Age is almost a matter of personal choice, and so it will remain until a little 'tell' like Coppa Nevigata, with its Neolithic, Bronze and Iron Age strata, is systematically sectioned. First, however, we need to know the span of life, and composition (with the total of storage pits, etc.) of a typical small Neolithic homestead. We now have the widest possible choice of site, and are forearmed with such ready-made site-plans as are seen on PLATE 2. In the test-excavation that is now so necessary, the surrounding ditches should be sectioned and related stratigraphically to the nearest clearly defined hut-enclosure, which ought to be stripped together with the ditch around it. This would provide material to define the dating of sites of this class, and also sample plans of huts and other structures which at present are quite unknown. This would be invaluable for the planning of future work, which logically should aim at stripping an entire

⁶ In the *Proceedings of the Prehistoric Society*, 1947.

ANTIQUITY

settlement in the manner of Köln-Lindenthal. The lessons to be drawn from the total stripping of the Iron Age farmstead at Little Woodbury, with its far-reaching repercussions on the social and economic history of its period and even on the demography of Roman Britain, are too recent to need further emphasis.

But there is much still to be learnt from the analysis and synthetic study of the corpus of settlements on the air photographs. Let us take for a moment, as laboratory specimens for examination, the representative sites shown on FIG. 1, and selected to show various recurring characteristics. They have been plotted from photographs of a scale of 1:15,000, and are here reproduced at a uniform scale for each. Plan A shows how close together they often occur, 500-800 yards apart, and how the superimposition of similar lay-outs indicates a long life and successive phases of re-construction. Plan B illustrates one of the most elaborate of the small sites which take advantage of the low escarpments round the plain. Portions of no less than 8 ditches, in two groups, are visible. There are numbers of such 'cliff-castle' types inland at the back of the plain. On Plan C is a typical village and a typical 'homestead' site. The concentric arrangement of the larger is only an emphasized form of the smaller. We do not yet know if there was a progressive growth in size, but I am inclined to think that both types were probably present from an early stage. The outermost enclosure of the smaller appears to have its counterpart in the similar feature (of uncertain extent) in the larger site. We do not know precisely how 'mixed' (7) was the 'mixed farming' of the Neolithic peasant in Apulia. This is one of the major problems to be solved by excavation. But, as already suggested, this outermost enclosure is probably a 'home farm', as it were, for pasturing herds at night or at particular seasons, or perhaps for fencing off cultivated garden land (no traces of internal field-boundary ditches have been seen). Plan D shows a settlement whose two inner ditch-enclosures closely resemble in size and plan those of the Neolithic village of Köln-Lindenthal. An escarpment site of the simplest sort is adjacent. In Plan E, which for its great area is extraordinarily complete, we have one of the finest examples of the village sites, which if it had possessed the powers of self-reproduction would verge on the proto-town. An oblique air photograph of this village taken by Williams-Hunt and myself, and a vertical for comparison, have already appeared (8). The settlement measures c. 500 by 800 yards overall, to which must be added another 700 yards for the outer 'home-farm' enclosure. Within are a hundred or more 'compounds' of varying size. It lies on level ground about 8 miles NE of Foggia, and, as with a number of sites, some little distance from a stream. It is common to find a small enclosure outside and adjacent to a large settlement, but since they are so thick on the ground this may have no significance. The ditches round the village are 15-20 feet across (measuring from the crop-marks which give a reasonably accurate idea, as I have tested by excavation). Their siting in pairs is a frequent feature, as will be seen on FIG. 2. On Plan F, we have a good-sized village following the contours of a low hill, but the 'compounds' inside are insufficiently clear to plot their positions, and their absence from some areas in the other sites is likewise due to crop-conditions. It would appear from Plan G, which resembles both Murgecchia and Murgia Timone, that we have

⁷ Compare V. G. Childe, *Dawn of European Civilisation*, 1947 ed., 226.

⁸ In ANTIQUITY, Dec. 1946, 196 seq., and Plates II and III. In 1945, I was able to have a trench cut through the ditch of a hut-compound inside a similar large sub-triangular village site. This produced, right down to the bottom of the square-cut ditch, a large quantity of the characteristic fine burnished wares and some painted pottery (decorated '*a fasce larghe*'), typical of the Apulian Neolithic, and both falling within Stevenson's Phase II. The ditch measured 7 feet in width at the top, and was 5 feet deep.

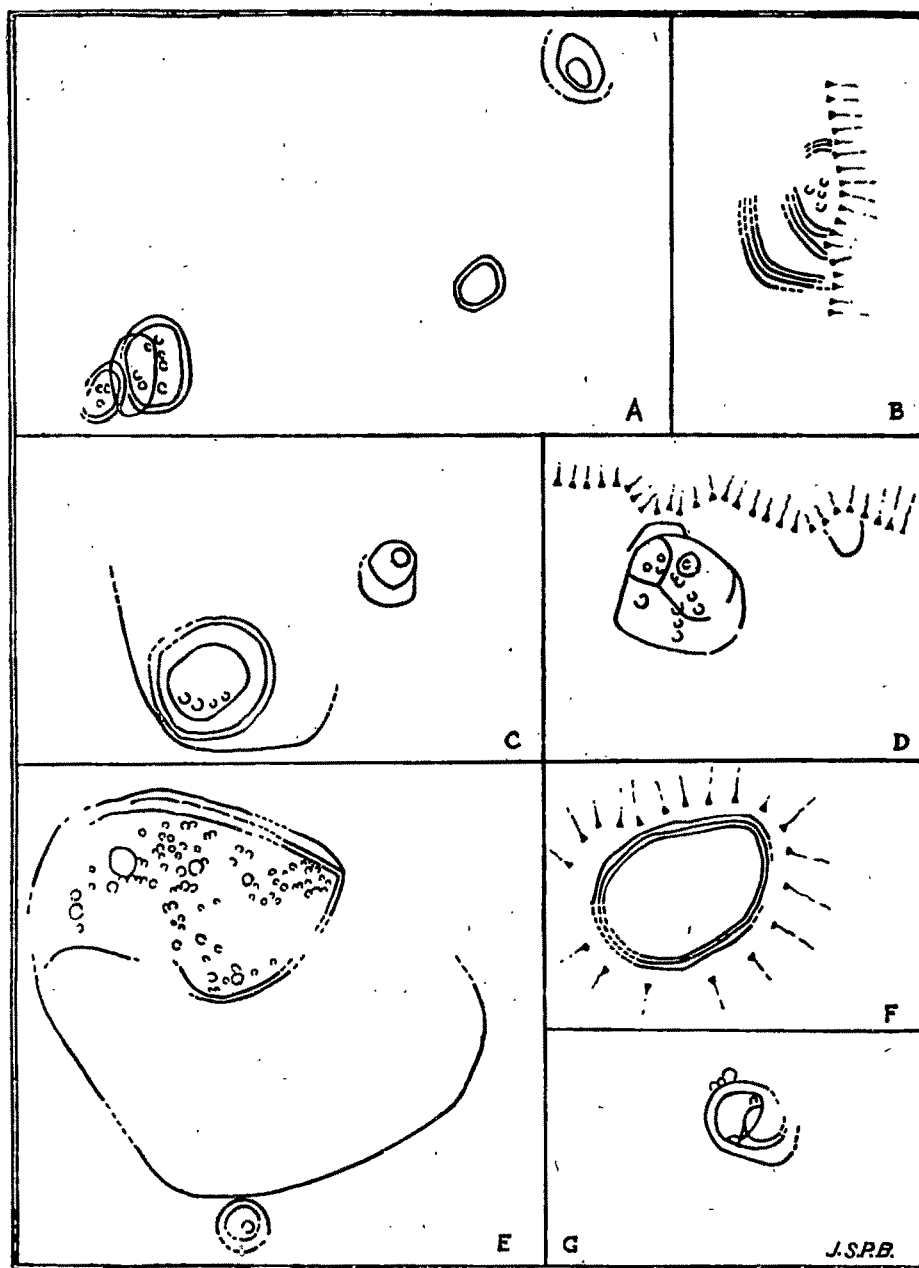


FIG. 1. COMPARATIVE PLANS OF DITCH-ENCLOSED HOMESTEADS OF NEOLITHIC TYPE AND OF VILLAGES WITH THEIR HOME-FARM ENCLOSURES; taken from air-photos. of the Foggia Plain and plotted on a uniform scale of about 1 : 20,000 to show the principal varieties of site

ANTIQUITY

a small group of 'compounds' outside the enclosure, but this seems to be very rare. Finally, on FIG. 2 we have a representative area from the prehistoric landscape, 5 miles NE of Lucera, on level ground on top of the 100 metre escarpment at the back of the plain. There are two large villages of similar plan and only 800 yards apart, which may well be successive. These measure some 800 yards across overall, and the 'compounds' seem to be confined within the inner pair of ditches which are some 15-20 feet wide, and for the most part about 50 feet apart, with 250-400 feet separating the pairs. The number of hut-compounds is much larger than it is possible to show clearly here. A 'homestead' type of site, with a typical example of the escarpment-edge siting, and the partial plans of two lesser villages (each 250 yards across overall), are also seen, besides a scatter of small circular enclosures (and there may well be more). The curving trackway, too, showing as a crop-mark (C-D-E), follows a pre-Roman route (see below).

The extraordinary degree of pre-excavation planning which can be obtained is illustrated by the small village with its hut-compounds, on PLATE II. These compounds are 14 in number, and we can see that we have here the total for the whole site. The area enclosed is c. 240 yards in diameter. The village ditches are 12 feet wide on an average, and those of the compounds from 8 to 20 feet (probably where re-cut). As is usual, the entrance to the village is not prominent in the plan, and is perhaps represented by the bulge in the line of the ditch (shown at A), if we follow the analogy of Ridola's sites on the Murge, near Matera. The area enclosed by the compounds varies between 40 and 100 feet across, and the area within the village ditches covers roughly $9\frac{1}{2}$ acres. A smaller site nearby, is revealed mainly by soil-marks in vineyards. A general picture of scale in relation to the modern landscape is given by the large courtyard farm, in the upper left-hand corner.

* * * * *

Following the procedure proper to analysis: to collect, compare and generalize, we now arrive at the third of these stages.

As we fly across the dry dusty plain, in its summer colours, and survey the beginnings of settled community-life over 4000 years ago, it is difficult not to feel a pang of admiration for these methodical, sturdy and ingenious peasants whose labours prepared the way for the evolution of cities and nations. They were orderly revolutionaries on the grand scale. A 'buried landscape' of primitive villages and homesteads is continually opening up to the view, and we examine this with something of the feelings and the precise familiarized eye of a District Commissioner in Africa today. Here are the first farmers in South Italy—more than 2000 years before Imperial Rome. By any reasonable analogy, it is unthinkable that the formation of the peasant character of this region was not deeply affected, far into later periods, by these six centuries or so of early farming. It is of the greatest interest to see how far these large villages could progress toward towns with very little, or no, metal or metallurgy; and as material for the sociology of prehistory they offer fascinating, if as yet untested, possibilities. As was explained in my earlier article, we may now infer a distribution of such Neolithic communities of fair density southwards over the Murge uplands, the Salentine peninsula (the 'heel' of Italy) and down to Taranto.

It will be seen that these discoveries stimulate as many questions as answers, and this is all to the good. One may speculate, for example, on the nature and function of the most distinctive feature of the group as a whole, namely their concentric ditches (for only a handful of *unenclosed* hut-compounds have been seen, and even these are in some doubt). They would have been appropriate to the new settlers, and needful

'BURIED LANDSCAPES' IN SOUTHERN ITALY

thereafter in a well-peopled but exacting region, in which competition for the better land must have been continuous. But the multiplication of the ditches (presumably palisaded) leads one to ask what considerations of tactics may have prompted it. Not many slingstones (9) have so far come from Neolithic sites in South Italy, but this is not conclusive for it required Professor R. E. M. Wheeler's systematic excavations to establish the relation of sling warfare and multiple ramparts in our own hill-forts. The 'homesteads', too, are a problem. Almost all show the combination of the 'domestic' and the 'farmyard' enclosures in their plan, concentrically or opening out of one another. Yet are a number of these temporary group-sites for seasonal occupation? It is impossible, as yet, to define the type socially or economically, but it must correspond to some particular form of human grouping (? the 'enlarged family'). They are freely spread all over the plain and not confined to a particular type of siting. The definition, by excavation, of one such site as an identifiable social group is an essential step in any future work, for generalizations on the social status of unexcavated sites are insidious and too easily accepted as facts.

But we may suggest that it was from this time, and in such a setting, that the familiar European combination of village and homestead originated, and the landscape begins to wear an aspect not unfamiliar. We must envisage it, too, against the background of the continuum of Neolithic peasants from Black Sea to North Sea. In spite of the regional variations which we can detect there is an over-riding and generic unity of culture transcending local differences. The study of these communities in South Italy is, therefore, bound to have a not inconsiderable effect on what we may term the 'international Neolithic'.

ROMAN FIELDS AND FARMS

The Roman landscape, which we are now able to reconstruct in outline, can be clearly distinguished on the photographs in its essentials. Since there is such a quantity of new material, we must be content here to dwell on certain of the more important features, and relegate much to a later and fuller treatment.

There are three topics of primary importance, which we shall deal with as follows:—

(1) Several elaborate systems of Roman land-partition by centuriation have been revealed, on the west and south sides of the Tavoliere, by the crop- and grass-marks of the boundary ditches. The *centuriae* (the square units of division) vary a little in size and orientation as between one system and another; but in all cases were demarcated by ditch-flanked roads. This chess-board landscape, which so vividly brings to mind a Tenniel drawing in the second chapter of '*Through the Looking Glass . . .*', is of the kind still well preserved in the existing field-pattern in parts of the North Italian plain. But *there* this incorporation in living field-systems has diminished its archaeological value. Continued use has preserved it but taken most of the life out of it, and only the bones are left. In Apulia, we are dealing with a Roman agricultural pattern which went out of use, and has been kept 'on ice' for us, as it were, below the ground. We cannot say, without excavation, exactly when these systems were laid down, nor in what manner and when they finally fell into disuse, vanishing from the surface. To piece together these systems from the traces shown on several thousand air-photographs requires both time and patience. It is apparent that separate and distinct areas of *agri centuriati* can be assigned to Roman cities round the plain.

(2) For the first time we are able to study the Roman farm-sites which lay at intervals along the roads, much as do the present State-planned farms in the same area.

⁹ See *Bull. di paletnologia italiana*, XLVI, 152.

ANTIQUITY

More than a dozen have been identified, and this number should soon be substantially increased.

(3) To the *centuriae*, which are the basic units of the land-division, we can now add the private field-boundaries within them, and even the pattern of the actual cultivation-trenches and pits, inside. The particular use of the latter can be deduced from size and arrangement, and by comparison of the lay-out with that recommended by such classical agronomists as Columella. A great acreage is covered by the patterns of these cultivation-trenches, and the problem of recording the detail is one new in archaeology.

We may conclude, therefore, by observing that never before have there been comparable opportunities for the study of Roman agriculture, *as actually practised*, or for seeing how far the agronomists' precepts were heeded, or for bringing a less rarified atmosphere, and some honest country mud, into the unresolved and abstract discussion of Roman professional terminology which has so often vexed the study of Centuriation. It has never before been possible to examine a Roman centuriated field-system 'three dimensionally' from surviving traces. Given this mass of new and detailed information a planned programme of excavation could be economically performed, with the assurance of important discoveries, and with an interest all the greater from being at the heart of the Roman World.

* * * * *

We must now turn to illustrations of these fundamental elements of the Roman landscape in North Apulia.

On FIG. 2, an area 5 miles NE of Lucera (*Luceria*), will be seen the junction of parts of two systems of centuriation, differently orientated and with the parallel ditch-flanked roads differently spaced. That on the left belongs to the assigned *ager* round *Luceria*, which became a colony of Rome as early as 314 B.C., as a strategically-placed thorn in the flank of the Samnites. In Augustan times it ranked as a *colonia*. Only the East-West roads (the *decumani*) seem to have survived clearly. They have a constant width of c. 12 feet and ditches c. 5 feet wide on each side, and in the area round *Luceria* average between 550 and 600 yards apart (or approximately 15 *actus*). The curly road which forms the joint boundary (C-D-E) is far from what one associates with the rigid gridded landscape of which we normally think. It measures c. 40 feet wide (including the ditches, one of which has been re-cut); and is very possibly of pre-Roman origin, chosen as an existing feature when the boundaries of the *agri centuriati* were decided upon. Careful search established that, here, the centuriation was not continued north of, and below, the 100 metre escarpment.

At this point we must refer to what has been done to collate the observed systems of centuriated fields on the photographs with those which are recorded as actually having existed in those invaluable agrimensorial writings entitled *Liber Augusti Caesaris et Neronis* and *Balbus Mentor*, otherwise known as the *libri coloniarum* or *regionum*. Pais¹⁰ has analyzed their entries in detail, and assessed their value much higher than did Mommsen but further consideration of the difficult problems of textual criticism must be omitted here. It is enough to say that the Roman sources give a number of details about the way in which land was divided round the towns in Apulia. In the preliminary interpretation of the air photographs, made some years ago and before consulting the

¹⁰ *Storia della Colonizzazione di Roma Antica*, vol. I, 1923, esp. p. 4 and p. 156. For a further discussion, see Rudi Thomsen, *The Italic Regions from Augustus to the Lombard invasion*, Copenhagen, 1947. His conclusions are of some value to our topographical researches, which we shall later collate with the written sources and inscriptions.

‘BURIED LANDSCAPES’ IN SOUTHERN ITALY

libri coloniarum, I noted centuriated field systems with significant differences in the orientation of their units in the neighbourhood of the following former Roman towns:—Troja (*Aecae*), Ortona (*Herdonia*), Ascoli Satriano (*Ausculum*), Canosa (*Canusium*) and Lucera (*Luceria*). The positions of these towns, on the first low escarpments at the back of the plain, will be seen on PLATE I A. It is of particular interest that these towns are, in fact, mentioned in the *libri coloniarum* as having their land regularly laid out, and

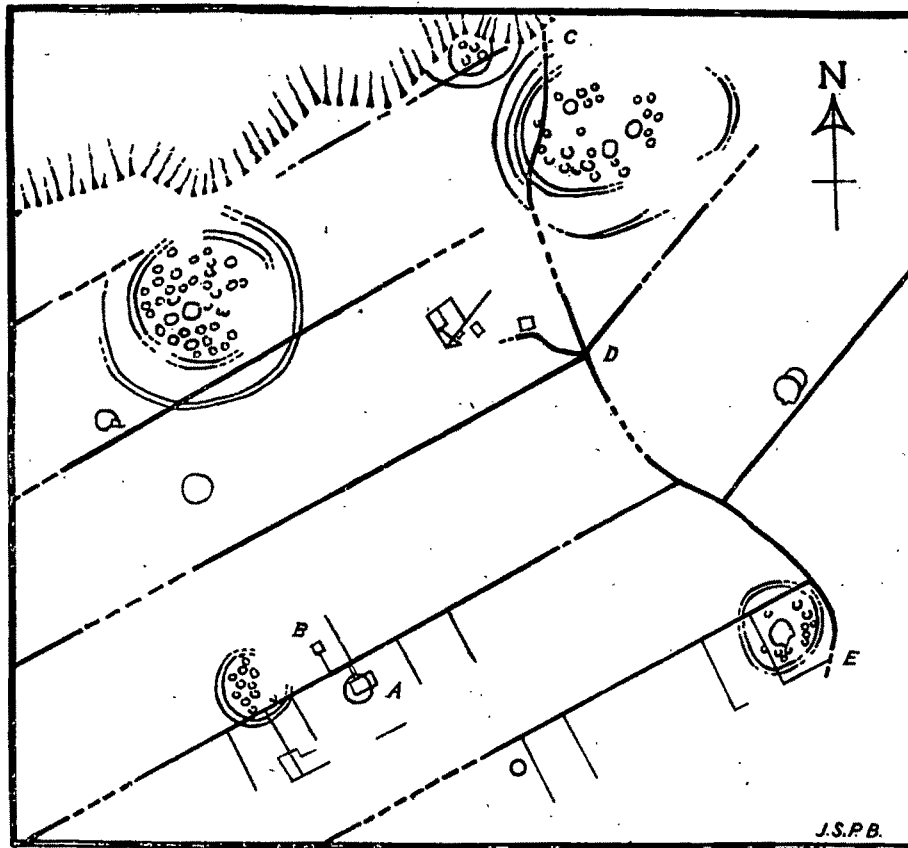


FIG. 2. AN AREA 5 MILES N.E. OF LUCERA, SHOWING A SPECIMEN AREA OF CROP-MARKS PLOTTED, WITH CIRCULAR NEOLITHIC VILLAGES UP TO 850 YARDS ACROSS AND THE BOUNDARY BETWEEN TWO ADJACENT SYSTEMS OF PARALLEL CENTURIATED ROMAN ROADS

Scale about 1 : 26,640 (1 inch = 740 yards)

details of their centuriation are given in several cases. But no trace of centuriation has yet been seen, on the photographs, between Foggia and the L. Salso (PLATE I A), and there is none round the site of *Arpi* (nr. Foggia) which in the 3rd century B.C. was one of the most important cities of Central Italy. Of the situation at *Sipontum* and *Salapia*, I cannot yet speak with certainty, but this preliminary statement on the collation and identifications already made may be of use. We now have in front of us the lengthy process of accurately positioning the constituent fragments of these centuriated field-systems, spread over many map-sheets of the 1 : 50,000 series.

ANTIQUITY

Next in order we may illustrate the farms. Let us first consider the two on PLATE III A (for their location, near Lucera, see FIG. 2, at A and B). These are connected with the Roman centuriated road by ditch-flanked drives. The larger (A) has a subdivided rectangular enclosure (490 by 150 feet), with an irregular dark central patch which may represent the site of a building. It is nearly superimposed on a circular ditched enclosure of prehistoric character (not *all* of the simple circular enclosures are necessarily Neolithic). On the opposite side of the road, at B, is a much smaller Roman enclosure, about 60 by 60 feet within. Several such enclosures have been identified just off the roads and connected to them, and they may possibly be equivalent in function to those little isolated buildings which today are found in this area among the vineyards, and are lived in temporarily during the vintage or occupied by a watchman who tends the grapes. This suggestion finds support from the fact that immediately behind the enclosure (at D) are rows of parallel Roman cultivation trenches, set 10 to 12 feet apart from centre to centre, which we have good reason to believe were for vine cultivation, as we shall see. There is another distinctive group of these at C.

We may take our review of the farms a stage further with PLATE III B. Here, once again, we have a Roman enclosure (a parallelogram 108 feet square internally, with a ditch 6 feet wide) (11) aligned parallel to a ditch-flanked centuriated road. Entrance gaps are visible at A and B. And *again* a circular enclosure of pre-Roman character occupies the same site. It is hard to reject the idea that some kind of continuity is in evidence in such cases. As before, the ground is flat, and there seems no other particular reason why these sites should be superimposed. If this could be confirmed by excavation the implications for the history of this region in the early period of Rome's authority would be of considerable interest; moreover, in the preceding centuries, the human landscape outside (12) the townlets is still dark indeed. It should be noted that, close at hand, there is a large area of cultivation trenches (6 to 8 feet apart from centre to centre) probably a vineyard adjacent to the farm, and aligned on a ditch connected with it. The large dark patch over it is due to modern stubble-burning, and the tracks of motor vehicles can be seen on the right of the photograph. With practice one can eliminate such extraneous features from further consideration.

Dissecting our buried landscape still more minutely we come next to the cultivation trenches and pits. Here is one of the most interesting aspects of all, for we are dealing with the Archaeology of the commonplace, indispensable but elusive and very liable to go unrecorded. It is through its attention to such detail, and its ability to reveal it, that 'dirt archaeology' can help the writing of classical history to take the next big step forward. These features are the works of the innumerable peasantry and yeomanry on whom the staple cornfields, vineyards and olive groves depended. It is right to recall those words of Professor R. E. M. Wheeler: 'The detailed history of an African farm recovered by careful excavation would throw a flood of light upon the chequered economic life of this vital province'. No less, it must be emphasized, is this true of Italy itself.

An examination of PLATES IV and V A will reveal the kind of detail that is awaiting investigation. On PLATE IV is visible (at A) the intersection of two roads (*limites*) delimiting

¹¹ i.e. about 1 *actus* across. One is reminded of the small Roman farm sites, enclosed by rectangular ditches, found from the air in Oxfordshire by Major Allen (see *V.C.H.*, Oxon., I, Plates XXI-XXIII).

¹² As T. J. Dunbabin has pointed out in *The Western Greeks* we know very little indeed about this aspect, and the social picture thus tends to be unbalanced.

‘BURIED LANDSCAPES’ IN SOUTHERN ITALY

the *centuriae* (13). These roads are *c.* 10 feet wide, with ditches *c.* 4 feet wide on either side. At B, exactly aligned along the road and unquestionably associated with it, is a large block of small regular crop-markings in parallel lines which represent elongated pits. About 36 rows can be seen with some 30 pits in each. These are each some 10 feet long, with an interval of *c.* 6 feet between those in each row, and a gap of *c.* 8 feet between the rows themselves. Another grouping of the same kind, across the road, in the same alignment and of greater length, can be identified on the original photographs but is obscured by modern agriculture. The spacing is rather too close (14) for olives, and this specialized pattern suggests a vineyard, orchard, or nursery plantation. We have never been really clear about the means of supplying the towns with their vegetables and fruits which played so important a part in provisioning the table. The holes had to be dug to take the tap-roots down to the subsoil, and perhaps fell to the lot of the *fossor*, the slave who specialized in digging by hand, not ploughing (15). Various crop-markings can also be seen at C and D.

PLATE V A displays another cultivation pattern, and one which covered large areas within our Roman centuriated field-systems. Again, it is fitted into the angle of intersection of two *limites* (at A). Here, the cultivation trenches are continuous lines, but stop just short of the ditch along the road, and a pathway (a feature advised by Columella) through them is clearly seen (at B). Areas of unsown land, and dark patches perhaps due to weeds, obscure the crop-markings somewhat, but enough is visible. The trenches are set *c.* 8 feet apart and measure *c.* 3 feet in width. The parallel with the modern vineyard, is very close; examples of the latter are indicated (intermixed with some olives) on PLATE II, at B. The opportunity of directly comparing the actual facts empirically by excavation with those principles of planting advised by Columella, and others, needs no stressing. In writing on the proper methods for planting vineyards, he recommended (Book IV) trenches $2\frac{3}{4}$ feet deep. These would certainly provide us with crop-marks, today. He suggests various methods:—‘Some set all their vines at 10 foot intervals in the form of a quincunx (16), so that the ground may be broken up by diagonal and cross ploughing in the manner of fallow land’. ‘But those who dread the expense of trenching the ground, and yet wish to imitate that trenching in some measure, run straight furrows to a width of 6 feet leaving alternate strips of equal width; then they dig the furrows and deepen them to 3 feet, and place the vines or shoots along the sides. Some make a furrow $2\frac{3}{4}$ feet deep and 5 feet wide; then leaving three times as much unbroken ground’ they cut another (Book III, XIII).

In conclusion we must urge that the evidence from the air-photographs goes far to show that there has long existed some misconception about the nature of Roman economy in this region of Apulia. The Tavoliere has indeed become accepted as a classical instance of Roman land given over to pastoralism and seasonal transhumance of flocks between the plain and the mountains. This may well still be true of the area which as

¹³ For their position on the map, see the vertical view of the scale model (Plate I, at I, near the foot of the Gargano). They would be part of the centuriated fields ‘*quae circa Montem Garganum sunt*’, mentioned in the *libri coloniarum*.

¹⁴ A modern olive grove can be compared at E.

¹⁵ For his activities, which included the deep furrows for drainage (*sulci*) mentioned in Justinian’s *Digest*, see M. Maxey, *Occupations of the Lower Classes in Roman Society*, Chicago, 1938, 79–80.

¹⁶ An arrangement in groups of five. Any five in the same position form a square with the fifth at the centre. Examples of this have been found on the Apulian air photographs.

ANTIQUITY

yet shows no traces of centuriated cultivation, between Foggia and the Lago Salso, but the picture is now very different for other areas of the plain and for the gentler slopes behind it. Here we find centuriated fields and abundant areas of cultivation trenches. It is indeed a warning against building arguments upon scrappy written evidence and inference. Apulia had been labelled pastoral, and this idea has persisted through many authorities (17). In her *Geography of the Mediterranean Region*, E. C. Semple wrote that in Northern Apulia, between the Apennines and Monte Gargano, the subsoil was impenetrable to the roots of trees, and that though 'useless for orchard or vineyard, it served well for pasture land' (p. 326). We can now go far towards peopling this landscape, not in a sand-table world of theory, but in a setting of actual fields and farms which provide unrivalled opportunities for the direct study of Roman agriculture.

THE MEDIEVAL LANDSCAPE

The important part played by Pastoralism in this region in the Middle Ages is well known. Archaeologically this can be studied not only in the *tratturi* which survive above ground (steadily being encroached upon by cultivation), but also in those already abandoned which show as crop-marks in the modern fields. These *tratturi* were broad tracks, State-owned, for the passage of immense flocks, and were demarcated on each side by a ditch. It is clear that the photographs can contribute much detail to this aspect of the archaeology of recent history.

However there are more important discoveries, for the medieval field monuments of this region are little known apart from those as well preserved as Emperor Frederick II's Castel del Monte. We have been able to identify 30 or more earthworks of medieval type, many of them quite large. Some are of motte and bailey construction. The history of the Normans and Hohenstaufen in this region is all too little known, and these earthworks should provide rich material. The need at the moment is for a thorough ground-survey to supplement the photographs, and for the documentation of the new sites from written record. In the latter, we have been most greatly helped by the authoritative knowledge of Miss E. M. Jamison. Since it is intended to devote special study to the medieval landscape of the Tavoliere, in a later article, it will be sufficient to illustrate the general nature of the new discoveries, as material for economic and social history. On PLATE V B is a site 13 miles NNW of Foggia marked on the map as Torrione del Casone, 'the large tower of the great house'. A large rectangular earthwork, with bank and ditch, is seen at A, measuring some 430 by 370 yards. Inside, besides a farm, can be seen buried traces of stone buildings, including the base of a tower. But the feature of special interest is the pattern of ditch-enclosed fields round about it. These are sub-rectangular in shape and vary from 50 to 450 feet across. They do not show traces of vineyard-trenching and so may have been intended for cereals. They are clearly associated with the radiating ditch-flanked trackways (C, D, E, F, G), which are also shown by crop-marks, but the large modern fields take an entirely different course.

Thus, for the first time we have the detailed plan of a medieval field-system, which at some later period went out of use and has thus been preserved in skeleton form, in the ground. With care and good fortune it may be possible to reconstruct the whole extent of the area round the earthwork that was thus intensively cultivated. The traces of these

¹⁷ It is accepted by E. H. Carrier in his *Water and Grass, a study in the Pastoral Economy of Southern Europe*, 1932, and has found its place also in M. Cary's recent book, *The Geographic Background of the Greek and Roman World*, 1949, pp. 140-1.

'BURIED LANDSCAPES' IN SOUTHERN ITALY

fields continue for three-quarters of a mile in several directions, and the cultivated area was thus quite considerable. This site is matched by others discovered on the Tavoliere. It lies only a mile or two to the south of the area of Roman centuriated fields at Casa Scoppa, shown on PLATE IV (the position is indicated on PLATE I A, at 1) on the level plain with the Gargano rising suddenly above it, magnificent in mountain wildness. Protected from the north and east winds, and well-watered comparatively, this corner of the plain gives a feeling of security, fertility and natural beauty, to which no early farmer could be insensitive. But this, of course, is also the very part of the plain which has always been thought of in terms of medieval pastoralism and almost its special preserve; although we knew from the documents that some villages did exist and that there were constant litigations as the peasant cultivators fought a losing battle against the encroachments of a State-sponsored pastoralism. Now, however, a re-interpretation of the economy and society of the medieval Apulian landscape will have to be undertaken, in the light of the new facts, and with much greater emphasis on its cultivation. We are in fact faced with an entirely new situation. As will be seen on PLATES V B and VI, this evidence can only be brought to light, in the first instance, from the air. Cut the crop, and you remove it until the next spring season, like a well-cleaned slate. But once recorded on the photographs, the course of the ditches and other features can be followed up on the ground, and excavated for dating material in the usual way. The study of the medieval pottery of this region is in itself a new subject, and trial-excavations at a site of the kind illustrated should provide us with an invaluable stratified type-series. At first it might be difficult to date, but everything has to have its beginnings if we really want new knowledge, and the situation should encourage rather than deter.

Now, let us turn to consider PLATE VI. This site is $3\frac{1}{2}$ miles south of Foggia, and its position is shown on the vertical scale-model (PLATE I A, at 2). The farm within it has perpetuated the name of San Lorenzo. As will be seen, it is a large earthwork with bank and ditch (still wet in places, to judge from the dark tone of the vegetation that marks its course). There are three clearly defined enclosures, A, B and C, which appear to have well resisted agricultural levelling. The innermost enclosure (A) measures about 340 by 230 yards, and the whole earthwork is 800 yards long overall. Though so large, very little is known about its history, but Miss E. M. Jamison has most kindly conducted preliminary investigations in the documents to remedy this, and informs me that it is recorded that, in 1237, the Emperor Frederick II built a *palacium* or *domus* at San Lorenzo near Foggia, causing a village of the Bishop of Troja's to be removed from that spot. It would certainly appear very probable indeed that this, and the truly formidable earthwork whose plan has been recovered at the farm called San Lorenzo, just outside Foggia, are really one and the same. Any addition to our knowledge of the magnificent but enigmatic Emperor, that 'Stupor Mundi', would be welcome, and the life and character of his court and followers are still imperfectly known, although among the most interesting in the whole Middle Ages. Archaeological investigations here, especially of the buildings within, might yield information of great value (18). Around the earthwork is a network of roads (D, E, F, G) and ditch-enclosed fields revealed by crop-marks and soil-marks, all radiating from it. These can be traced for three-quarters of a mile in several directions, though some may have belonged to the earlier village. Modern boundaries take quite a different course (for example, the estate-boundary at x-y), and patterns made by harvesting, etc., have to be eliminated (for instance the field being cut, at z), but the small,

¹⁸ Compare Professor Vogt's excavation of the Dark Age stronghold and Ottonian palace of the early Middle Ages at Zurich; see *Der Lindenhof in Zürich* (Füssli, Zürich, 1948: 30 Swiss francs).

Problems and Policies

by F. T. WAINWRIGHT

PERHAPS at no time more than the present have British archaeological studies had such need of a stocktaking¹ (1). An occasional stocktaking of knowledge and techniques is essential to the successful advancement of any study, and the Council for British Archaeology has undoubtedly stimulated archaeological studies by the publication of the first part of the *Survey and Policy of Field Research*. Under the joint-editorship of Professor Hawkes and Professor Piggott many of Britain's archaeologists have co-operated to review the present position and the future development of British archaeological studies. The volume is divided into two chapters; the first surveys our archaeological knowledge, and the second indicates how the major problems may best be tackled. Its span in time appals the mere historian. It ranges from the Palaeolithic Age to the 7th century of the Christian Era, from the so-called 'eoliths' to the so-called 'Kentish jewelry'. No single scholar would have been competent to discuss all the problems raised, and no single scholar is competent to criticize the work of this team of specialists.

The *Survey and Policy* is, by definition, archaeological. It is not the function of its authors to deal with kindred studies such as geology, dendrochronology, pollen-analysis and, later in the time-scale, place-names and the literary sources. Yet throughout the work one is conscious of the interdependence of these studies. The emphasis may shift from age to age, but the full picture of the past will always elude the single-minded approach of the single-minded specialist. This is extremely well illustrated in the earlier sections where the problems are as much geological as archaeological. It is equally well illustrated in the section devoted to 'Anglo-Saxon England' where the inevitable exclusion of literary and linguistic evidence leaves us with an archaeological residue that is 'surprisingly slight'. Though bound to limit their attention as far as possible to archaeological knowledge and to archaeological problems, these specialists lose no chance of driving home the vital point that pictures of the past are many-sided and that different techniques are required to reveal their different facets.

The ignorant layman and the semi-ignorant spectator cannot fail to be impressed by the masterly summaries of knowledge which are laid before them. Some summaries are more attractive and more convincing than others. Perhaps the most useful are those that list most possibilities and fewest facts, for at the present stage it is not wise to close too many doors even though the layman may cry for certainties. Masterly, too, is the discussion of the chief problems in British archaeology; concrete proposals are made for their solution and, occasionally, practical recommendations on method are added. Enough is said to sketch the lines along which the problems may be attacked, but not enough to encourage the untrained amateur, if he needs any encouragement, to indulge in an orgy of destructive digging.

¹ *Survey and Policy of Field Research in the Archaeology of Great Britain*, Part I, p. 9 (published by the Council for British Archaeology, London, 1948).

ANTIQUITY

Throughout this volume excavation-policy is discussed with a caution that is wholly admirable. It is agreed that certain sites should be preserved intact and unexcavated, for it is recognized that a programme of excavation should take into account the limitations of present knowledge and techniques as well as the relative importance of the problems themselves and the menace of destructive forces in an industrialized community. And the untrained amateur is frequently reminded, directly and indirectly, that unskilled excavation, however enthusiastic, is a dangerous and a damnable thing. The need for trained and experienced supervision of excavation is stressed, and the urgency of this need deserves special consideration at a time when the professional-versus-amateur controversy, always smouldering, has recently burst into flame.

Apart from scattered references to the menace of the untrained, there is a careful introductory survey which deals with modern tendencies, especially the increasing complexity of these studies, as they affect the amateur. And one of the joint-editors has elsewhere delivered a statement on the functions of the amateur (2) which has evoked some bitter retorts (3). A certain amount of heat, not all reflected, has also been generated in local societies where the amateur element ranges from high authority to stupid ignorance. Every trained scholar, archaeologist and non-archaeologist, must surely support the demand that valuable and irreplaceable material be saved from destruction by the incompetent and the irresponsible. Archaeologists, historians and place-name scholars are all plagued by amateur frivolities until they wish they were nuclear physicists, a class honoured by the State, favoured by the Exchequer and apparently untormented by the cranks. But whereas the historians lose only their tempers (privately, one hopes) the archaeologists lose also their material. And that is serious.

It would seem, therefore, that the amateur and his place should be listed separately as a problem, but a proper definition of the term 'amateur' would remove much heat from the controversy and would leave behind only a harmless and amusing warmth. What do we mean, in this connexion, by 'amateur'? The distinction based upon the assumption that a 'professional' pursues as a livelihood what the 'amateur' pursues as an unremunerative hobby suffers from a double weakness. It was always a poor distinction, and it has become ridiculous with the introduction of a week of five short days for most 'amateurs' and with the rapid expansion of teaching and administrative duties for most 'professionals'. Very few scholars earn their living by scholarship alone, and very few become scholars merely in order to earn a living. The old livelihood test must go, and with it must go (in this connexion) the terms 'amateur' and 'professional'. We are not concerned with the 'amateur' and the 'professional'. We are concerned on the one hand with the trained, the skilled and the experienced and, on the other hand, with the untrained, the unskilled and the ignorant. We should not say 'amateur' when we mean incompetent, and we should not say 'professional' when we mean scholarly.

Archaeological studies are to-day so complex that no man—not even he who might devote twenty-four hours a day to it—can range authoritatively over the whole field. Specialization has come to stay, and that is both beneficial and inevitable. Even to-day, however, the 'amateur' can hold his own if he limits his field, if he acquires the skill, the training and the experience, and if he has the time, the patience and the ability to achieve a mastery of his subject and of its background. These are the criteria—not

² Stuart Piggott, 'Archaeology and the Amateur' (*The Archaeological Newsletter*, No. 1, April 1948).

³ See subsequent issues of the *Archaeological Newsletter*.

PROBLEMS AND POLICIES

whether or not one is constrained to contribute to the Exchequer for the doubtful privilege of calling oneself a 'professional'. The dividing line is not between the amateur and the professional; it is between the trained and the untrained, the skilled and the unskilled, the competent and the incompetent.

The best amateur and the best professional will be found together on the same side of that dividing line. It is true that incompetent professionals are comparatively rarer than incompetent amateurs, but that is no cause for either pride or shame. It has a simple and obvious explanation: the professional's career depends (to some extent) upon his competence as a scholar, whereas the banks and the breweries are unperturbed if their servants choose to be bad archaeologists instead of bad golfers. The controversy has generated a quite unnecessary heat. Let not the cries of the incompetent and the ignorant divide the ranks of the competent or destroy the recognized standards which have always bound together the good professional and the good amateur. If an attack be worth the effort, let it be directed by both against the ignorant and the incompetent in our local societies, who, not content to watch and to subscribe, try to impress their honest brethren with their own empty claims to scholarship.

It is often argued that the amateur, freed from the dread of having to pay for his mistakes, introduces a stimulating unorthodoxy into archaeological opinion. The amateur's claim to a monopoly of unorthodoxy might be disputed, but no one will deny the importance of reviewing frequently one's evidence, opinions and methods. Unorthodox approaches, when coupled with a mastery of technique and an awareness of its limitations, are both welcome and stimulating. But without that mastery and that awareness unorthodoxy is apt to be ridiculous. Any fool can be unorthodox, even original, but unorthodoxy and originality in themselves have only an amusement value. On this point, as on all others, there seems to be no quarrel between the professional and the amateur as such. If there is a quarrel it is against the blunderingly incompetent and the presumptuously ignorant. Not enough hard words have been directed against these classes which have too long confused themselves with those distinguished amateurs who hold secure places in the highest ranks of scholarship.

The amateur as such is no problem. The incompetent amateur is a problem when he destroys important sites or when he gains control of local societies which for the most part are composed of sane and cultured men and women who are sincerely interested in scholarship and the promotion of scholarship. Incompetent and irresponsible individuals can do much harm by wasting funds on trifling publications, by undermining standards and by generally misleading the public (which is usually too busy to distinguish between the genuine and the fake). Such individuals should be imprisoned or, preferably, buried. But they should not be elevated to the rank of major problems.

There are many greater problems, and some of these, already apparent in the first part of *Survey and Policy*, will surely dominate the second part. *Survey and Policy* is concerned with archaeological problems and with archaeological policy; non-archaeological studies, already important for prehistory proper, are vital to an understanding of the Dark Ages. This is clearly demonstrated by the later sections of Part I. A survey of the archaeological problems of the Dark Ages is urgently required, but its value will be limited. Surveys of non-archaeological problems are also required and, finally, a survey which co-ordinates and integrates the various problems and the various lines of approach. This is no criticism of the principles underlying *Survey and Policy*—a wider approach would be outside the scope of the Council for British Archaeology. On the contrary, the example set by the Council might with advantage be followed by other groups of scholars. It would be extremely useful to have the various sets of problems

ANTIQUITY

surveyed and to have even tentative policies available for consideration and discussion (4). Unfortunately at present there is no organization to stand aloof from sectional rivalries and prejudices and to carry out a co-ordinating survey. A Council for or an Institute of Dark Age Studies is required. An Institute or a Council or an Academy of 'Dark Age Studies' (forbidding title), 'Anglo-Saxon Studies', 'Anglo-Scandinavian Studies', 'Anglo-Celtic Studies', or simply 'Pre-Conquest Studies'—the name is not very important. It would be wise to include Celtic studies, and it would be wise to avoid in its name words like archaeology, history and philology. But the chief thing is that it should exist to encourage and co-ordinate all lines of inquiry likely to lead to the elucidation of the problems of Dark Age Britain.

Dark Age problems may be divided roughly into three broad classes. There are the gaps in our evidence; there are the problems of technique, and there are the wider problems of interpretation which arise from the need to translate our different kinds of evidence into history. The second part of *Survey and Policy* (which, incidentally, will cover the Medieval and post-Medieval periods as well as the pre-Conquest period) will no doubt sketch the archaeological gaps in our knowledge with the same mastery of touch as that which is displayed in Part I. We shall probably be told, for example, how we may best obtain archaeological evidence of the expansion of the English settlements and of racial boundaries in Scotland, how we may find or recognize at least a few heathen Scandinavian burials, how we may add to our knowledge of weapons and agricultural implements, village sites, ecclesiastical and domestic architecture, industrial developments and artistic achievements. We may hope for new lines of approach to the study of fortifications and battle-sites. But many gaps will remain that no archaeologist will be able to close. There are political, social, economic, artistic, literary and linguistic problems which archaeology alone can never solve. Nevertheless, we should like to see these non-archaeological problems clearly indicated, and we should like to see laid down the broad lines of policies aimed at their solution. We should also like to see all these problems, archaeological and non-archaeological, brought together in a single discussion; but until some co-ordinating agency is developed the interested spectator must remain in his fog.

The second set of problems, those of technique or method, are too complex for detailed examination in a general discussion. Excavation embraces the primary, though not the only, techniques of the archaeologist. It is only after he has recognized his problem and selected his site that the skill and technique of an excavator are brought into play. Excavation techniques are undoubtedly capable of great improvement, but their development in recent years has been spectacular, and to-day they present perhaps the most encouraging aspect of archaeological studies. It is as inspiring to watch a master of excavation technique as it is distressing to watch a bungler. We cannot appreciate a master's skill by reading his reports; we must see him at work in the field and we must experience his difficulties. Without some experience of excavation one cannot hope to assist in the gradual perfecting of excavation technique; a historian may guide an archaeologist to a site and he may perhaps greatly assist in the defining of problems against their background, but the physical attack upon the site must be left to the trained excavator. Those who have taken the trouble to examine different methods employed

⁴ Something of this nature was attempted for place-name studies about twenty-five years ago, i.e. *Introduction to the Survey of English Place-names* (English Place-name Society Publications, Vol. I, Part I). In this series of chapters, which cover the various aspects of place-name study, the authors set out 'to state the present state of our knowledge and indicate the lines along which the possibilities of future progress lie'.

PROBLEMS AND POLICIES

and to watch sympathetically different excavators at work are perhaps best qualified to comment on technique. Such observers believe that, though there are many obstacles still to be overcome, the development of the technique of excavation is in safe hands.

Non-archaeological studies have their own techniques. The historian may be engaged, for example, in stripping off the layers that have gone to make up the Anglo-Saxon Chronicle. Or perhaps he is engaged in testing the authenticity of charter material. The aim is fundamentally the same, but the spade is no use. Different techniques are required, and they often demand a skill and a knowledge outside the normal equipment of the historian (5). The student of place-names, too, is engaged in stripping off layers of phonological development and in searching for underlying and earlier structures. These and other studies have made considerable technical advances in recent years; some day even the Anglo-Saxon Chronicle will be sufficiently stripped or dissected to enable us to see the relationship to each other of its various sections and its various manuscripts. Unfortunately the spectator sees hardly any of this game; full realization of technical difficulties comes only from active participation. All these techniques are concerned primarily not with the interpretation of evidence but with the accumulating, the checking and the defining of evidence. It is not easy to see how they can be practised effectively except by trained specialists.

The third and most formidable group of problems arises from the fact that it is ultimately necessary to integrate these various accumulations of evidence into a single historical synthesis. It is extraordinarily difficult to co-ordinate the work of specialists. As a rule the historian, the archaeologist and the philologist distrust each other's methods. The philologist will often denounce the historian as 'inaccurate' or 'unscientific', and what he has to say of the archaeologist is often as vivid as it is unprintable. The archaeologist, in his turn, may declare that the historian is too cautious to reach useful conclusions and that the philologist is too rigid to reach any conclusions at all. Sometimes the historian appears to stand between the two: in matters of general interpretation he may consider the philologist too cautious and the archaeologist too careless. One will appeal to his 'academic standards' and the other will appeal to his 'common sense', but the object of this pleasant wooing frequently finds himself the victim of a disconcerting cross fire of abuse. We have all heard in private, and occasionally in public, mutual complaints and denunciations uttered in bitter rage, academic indignation or sly geniality. That specialists distrust the methods of other specialists is a fact that must be faced.

This distrust is not universal, of course. There are historians not frowned upon by the archaeologists and there are historians who have secured a cessation of abuse, if not positive praise, from the philologists. There are philologists who willingly bring their special techniques to the elucidation of historical problems and who do not openly condemn archaeology as an unacademic pursuit. And there are archaeologists who are not unimpressed by the arguments of historians and philologists. There are even a few scholars who move, unmolested and even respected, in two of the three camps. Nevertheless the barriers exist, and there are similar if less obvious barriers separating the several narrower paths that lie within these three broad avenues of approach.

How has this situation arisen? It has arisen chiefly because each scholar is dealing with a different kind of data, and because each different kind of data requires for its interpretation different methods of inference, different rules and techniques. An instance

⁵ See, for example, Miss F. E. Harmer, 'Anglo-Saxon Charters and the Historian' (Bulletin of the John Rylands Library, Vol. 22, No. 2, October 1938).

ANTIQUITY

of assimilation revealed by a place-name form, for example, is not the same as a piece of pottery, and neither is the same as an entry in a chronicle. In none of these cases is the full historical significance of the evidence either obvious or near the surface. The most one can do is to draw from each a tentative historical conclusion, and disputes often arise because each scholar brings his own methods to the problem and tends to criticize the more, or less, definite conclusions obtained by other methods. The gradual development of the techniques of attack (i.e. against a site or a manuscript or a collection of place-name forms) has not been accompanied by a comparable development in the techniques of interpretation. Indeed the specialization which has so greatly improved one set of techniques has thrown new difficulties in the way of the development of the other. The time has passed when a single scholar could range with equal authority over a vast period. He now knows he cannot speak with equal claim to attention on every aspect of even a short period. Specialization, however, has come to stay. It has brought great advances, and it will bring more in the future. But it has emphasized and increased the difficulty of integrating the different bodies of evidence.

Dark Age studies receive the full impact of these differences in evidence and technique. In later ages historical information is usually too precise to tempt the archaeologist to ignore or reject it. In the prehistoric ages he has a clear field because there is no historical evidence to be considered. Many non-archaeologists may feel that the archaeological material of Neolithic Britain, for example, is insufficient to bear the superstructure of interpretation often raised upon it, but they refrain from criticism because a protest would look like interference. In the Dark Ages it is different. Here there is historical information and, however lacking in quantity and in precision, it must be taken into consideration. One of the greatest problems in the study of Dark Age Britain is how to reconcile current interpretations of archaeological material with current interpretations of literary and linguistic material.

It is not wise to write of battles and conquests when one's only evidence consists of changing designs on pottery or changing customs in the disposal of the dead, and it is not wise to see political convulsions in vowel sounds. Political events frequently produce artistic, social and linguistic changes, but to infer the former from the latter is like shooting in the dark. For the Dark Ages at least it is also unwise to build theories even on the apparently explicit statements of chroniclers until these statements and their sources have been carefully examined. An entry in a chronicle is not the same as what we may loosely call a historical fact. It can be demonstrated as a fact, for example, that in a certain Cambridge college there is a certain ancient manuscript which contains an entry which records that in A.D. 571 a certain Cuthwulf defeated the Britons and captured four named towns. We are not thereby automatically entitled to assume that in 571 the West Saxons advanced for the first time into territory hitherto held by the Britons. Without an exhaustive study of the source we are not entitled to assume that Cuthwulf won a battle against the Britons in 571. We are not even entitled to assume that there was a battle or that Cuthwulf existed except in legend. An exhaustive study of the source is itself only the beginning of a process which, if ultimately successful, will permit a historical conclusion—never, it should be noted, anything so precise as a mathematical proof. One must be cautious. Some archaeologists (and some historians) too readily accept such entries without adequate examination. Others too readily reject them without adequate examination. The examination may require the application of specialized techniques or the application of techniques not yet developed, but no such difficulties can justify a premature assumption that a question is settled. A manuscript may be material, but the statement that it contains is not like a piece of pot; it cannot be picked

PROBLEMS AND POLICIES

up, measured, washed, treated with chemicals and labelled. Apart from such rare oddities as explosive cigars and oranges of soap, the archaeologist's material is direct evidence not of a state of mind but of a 'culture' and of a person's practical skill and (perhaps) aesthetic interests. A statement in a manuscript, unlike a piece of pot, may be true or false: it may represent an accurate summary by a person who knew the truth or a guess by one who did not; it may even represent a deliberate attempt to deceive. It is direct evidence only of a person's state of mind, and (if we ignore the possibility of an attempt to deceive) a belief that a certain proposition is true is only indirect and often very weak evidence for its actual truth. Not all archaeologists have yet grasped these annoying but essential characteristics of historical evidence. The application of the wrong technique is as useless as the application of no technique.

It is unwise to propose a new series of political events from an insufficient quantity of the wrong kind of evidence merely because the right kind of evidence happens to be lacking. It is even worse to do this when it requires the rejection of the right kind of evidence merely because the latter is imperfectly understood or apparently irreconcilable. But disputes can also arise from attempts to bring two kinds of evidence into close association. It is obviously important, for example, that archaeological evidence and historical evidence should be brought together, but when the evidence itself is meagre and intractable it is easy to make hasty and unconvincing associations. The simplest forms of such attempts at association are attempts to identify known archaeological evidence of battles with known historical conflicts, known burials with known historical figures, and known archaeological material with known historical events. Both archaeologists and historians spend much time working out and writing up such identifications. When they are successful their importance is great, but unfortunately the evidence for an identification of this kind is not always convincing to all parties. What to one is a reasonable probability may be a wild and unsupported guess to another. The meanings of 'possibility', 'probability' and 'proof' seem to have become blurred in the minds of some writers. When an archaeologist has shown that a certain burial belongs to a certain age and to a certain type he has opened the door to a number of possibilities. It is possible that he may have found the grave of a known historical figure who died in that area at that time. But it is also possible that he has found the grave of a person whose name has escaped preservation in the meagre historical sources. There were rather a lot of people in the Dark Ages whose names are not recorded in charters and chronicles. A possibility is no more than a possibility until further evidence turns it into a probability. We can all think of cases in which archaeologists and historians have leaped across the chasms that separate possibility from probability and probability from proof. It does no harm to throw out a possibility, of course, provided that it is properly labelled, but any theory built upon it cannot be stronger and will probably be weaker than its dubious foundation.

The techniques of interpretation are indeed in their infancy. It is easy for the historian to condemn the archaeologist's conclusions, and it is easy for the philologist to condemn the unscientific slovenliness of both historians and archaeologists. But all have to adopt some kind of historical method in so far as in the Dark Ages there is historical evidence to be considered and in so far as all seek a historical sequence of cause and effect in human activity. When the archaeologist thus turns historian he should remember the complexity of historical evidence. The philologist, on the other hand, might be more patient if he remembered that historical and archaeological evidence defies a strictly scientific handling. The philologist possesses a body of rules to assist him in tracing the origin and development of his vowel sound, but each site and each annal present to the archaeologist and to the historian new problems which cannot be

ANTIQUITY

approached through rules based upon other sites and other annals. Each is a law to itself, and results are correspondingly more tentative. Different techniques of attack, devised for different kinds of evidence, have made it more difficult for their exponents to overcome the problems of co-ordination.

In the problems of interpretation and co-ordination a specialist can learn from other specialists. Philologists have developed a technique and they practise it. Historians will profit from a study of philological methods, but they cannot hope to apply philological precision to historical evidence which, incidentally, includes historical deductions from linguistic and archaeological material. Therefore historians have developed their own techniques—it's a pity they don't use them more often. The archaeologist can learn much from history and philology, but in the last resort his technique must be his own because, even though he seeks a historical synthesis, his material is archaeological, not literary, linguistic or historical. It is clear that the nature of the material influences both techniques used in examination and techniques used in interpretation.

The archaeologist needs help not abuse. His chief problem at the moment is how to develop for the interpretation of archaeological evidence a technique which recognizes the distinctive characteristics of archaeological evidence and at the same time permits the incorporation of his conclusions in a convincing historical synthesis.

It is important that specialists should understand each other's techniques and each other's difficulties. In the Dark Ages each is brought into close contact with the other's material, and blunders often result from attempts to handle unfamiliar kinds of evidence. The historian and the archaeologist may go astray if they attempt to use place-name evidence without first considering its limitations, and the philologist may misunderstand archaeological evidence unless he understands its nature. One will never be able to dictate to the specialist in his own field, of course, but one should try to understand his problems before trying to use his material. It might be argued that the specialists should be left to pursue their own lines in their own ways and that co-ordination should be limited to exchanges of questions and answers. Little would come of that. We all know how unintelligible and how irritating both questions and answers can be. An archaeologist who seeks help from a philologist without understanding his techniques and difficulties will infuriate him with questions that are either ridiculous or unanswerable. And the philologist, however anxious to help, will produce answers that are useless unless he understands the archaeologist's problems. We have all suffered from each other. A sympathetic understanding of the other man's technique is essential before one can ask him intelligible questions or give him intelligible answers. This seems to lie at the root of successful co-operation and at the root of effective co-ordination.

How can co-operation and co-ordination be best achieved? Some scholars have always taken the trouble to peer over the barriers that separate their own studies from the studies of others. Specialization is increasing the height of the barriers, but no doubt there will always be scholars big enough to see over them. Anything that encourages specialists to improve their general equipment without impairing their special equipment deserves to be fostered. It might encourage co-ordination, for example, if Dark Age studies were introduced into the universities as a separate composite subject for attention. Or a stronger public interest might follow the creation of a 'Council for Dark Age Studies' on the lines of, but with wider scope than, the Council for British Archaeology.

It would be possible to create a university department devoted to the study of the Dark Ages in Britain, but it would not be easy to staff it with men who were not mere historians or mere archaeologists or mere philologists. It would not be easy, but it should not be quite impossible. As a teaching department, however, it would be a

PROBLEMS AND POLICIES

doubtful asset. Courses and examinations do not themselves produce historians, archaeologists and philologists, and a new course which set out to produce a mixture of the three would probably produce nothing. To be effective it would have to be a post-graduate course for students who had spent a dozen years or more familiarizing themselves with the material and processes of its constituent parts. And for these students it would not be necessary. As an undergraduate course it would be no more than the softest of soft options. The department would have little value as an instrument for training the young and it would have less as an instrument for instilling into specialists an understanding of the difficulties and methods of other specialists.

Active research alone can produce that mutual understanding. It seems reasonable to demand that the historian who aspires to use archaeological or linguistic evidence should first undergo an appropriate training. That training will not be found in books and in lectures; it is obtainable only in the trench, the museum and the library, in grappling with a problem on the other man's ground and with the other man's weapons. Admittedly this training cannot be obtained in one year or in two, and admittedly he who pursues such a policy will still find himself dependent upon the specialist. But he will add to his own equipment, he will be able at least to ask intelligent questions, and when he attempts to co-ordinate evidence he will stand on safer ground than the specialist who has never wandered from his own field. Finally, it is possible that, backed by his own specialized techniques, he will add to the techniques of the other specialists with whom he has worked.

Such wandering specialists will naturally make their own contacts and their own arrangements. They have always done so, and they need no council or institute or department to give them a lead. But an organization of some kind would assist them in their efforts to co-operate with each other, and by its very existence it would stimulate interest in Dark Age studies. An institute or department devoted specifically to the many-sided problems of the Dark Ages would be more than a centre for discussion and a pool for information. It could attempt much that individual scholars, however enthusiastic, cannot attempt until they are drawn together within a single organization. A research institute staffed by trained historians, archaeologists and philologists, all bound together by a common interest in Dark Age problems and by a common experience of the different lines of approach to them, could formulate a policy and could direct research activity into its most useful channels. The trained archaeologist would devote some of his time to accumulating and examining archaeological evidence with specific *historical* problems in mind. The historian would devote himself not only to using archaeological evidence for historical purposes but also to accumulating historical evidence for a planned attack upon *archaeological* problems. It has never yet been done. The philologist would bring his material and his methods to bear not only upon linguistic problems but also upon historical and archaeological problems. These few men, inevitably few, could go far towards co-ordinating different and apparently divergent accumulations of evidence. Each would be a specialist in his own field but—this is important—each would have had experience in other fields.

It could be done. Three or six men could do much if they were brought together in this way, and they could train assistants at the same time. The historian who had accumulated evidence for the identification of a lost site would have expert advice at his elbow; he could have his linguistic evidence examined and he could have his theory put to the final test of the spade by men interested in the same problems. To-day if the historian cannot fire the interest of a busy excavator he must either mutilate the site himself (for the spade alone can prove or disprove his theory) or he must wait until

ANTIQUITY

some Bronze Age specialist accidentally mutilates it for him. In either case the vital evidence may well be lost forever. The archaeologist, in his turn, could call for precise historical information instead of having to accumulate it for himself from out-of-date textbooks. An institute could secure planned and permanent co-operation, and that surely would be more fruitful than the casual mutual assistance upon which we depend to-day. Moreover the historian would find that his experience as a 'dirt archaeologist' gave him incalculable advantages over historians who had seen excavations only from well polished shoes. And the historian who managed to see place-names through the eyes of a philologist would use them the more effectively. Likewise the archaeologist and the philologist would gain by close association with the historian, his material, his problems and his methods.

Until such an institute provides a centre for Dark Age studies all scholars who are interested in these problems would be well advised to assist at excavations, to try to see place-names as philological phenomena, and to consider carefully the historian's pre-occupation with questions of source, bias, reliability, etc. The aim of each should be to improve his own equipment, not to compete with other specialists.

A Council for Dark Age Studies, created on the model of the Council for British Archaeology, is not the same thing as a research institute. It would have a different task, the stimulation and organization of a wide and interested public. Archaeologists can teach other scholars much about salesmanship. To-day the public are the patrons; they provide, directly or indirectly, equipment, departments, funds and, if properly encouraged, lavish journals. They have a right to be taken into the confidence of scholars, they are anxious to be interested, and they are often willing to assist. It is the duty of scholars to produce readable summaries of their work, and archaeologists have shown that it pays. Separate reports surveying the non-archaeological problems of the Dark Ages, followed by a combined co-ordinating report, and all written in simple lucid prose, have been envisaged above. They would be of limited value to scholars actively engaged in research, but they would dissipate the mists of legend, ancient and modern, with their shades of long-haired Druids flitting whitely through scenes of midnight slaughter and mid-day revelry. The mists would be blown away, and readers would see the Dark Ages as a period of fascinating and interlocking problems, a period which calls for the best of modern techniques and which offers great scope for those who are interested in it. They would find from these reports that modern methods are more satisfying than ancient legends, and they would find in them clues which would enable them to reject many of the modern legends which pass for history. And no doubt they would derive considerable enjoyment from the spectacle of specialists entangled in academic webs of their own weaving.

The taking of Le Krak des Chevaliers in 1271

by D. J. CATHCART KING

THE great Syrian fortress of Le Krak des Chevaliers, the best known, as it is on the whole the finest and best preserved, of the Crusader castles, has recently been made the subject of a brilliant and exhaustive study by M. Paul Deschamps (1); taken in conjunction with earlier notices by Rey, Van Berchem, T. E. Lawrence and others, this has left archaeologists in an exceptionally favourable position with regard to their knowledge of this castle. A few objections on points of detail might be raised to Deschamps' conclusions, but it appears to me that there is only one important question on which further clarification of our existing knowledge is needed: the reconstruction of the siege and capture of the Krak by the great Mamluk Sultan of Egypt, Bybars, in April 1271. The accounts given by the Arab historians of the period are not perfectly clear, and the explanations offered by Van Berchem and Deschamps do not appear to be the best possible, in view of the evidence of the fabric itself. In order to obtain a sounder idea of what occurred in the siege, it will first be necessary to consider the buildings and their history in brief.

Le Krak des Chevaliers stands on a foreland of the bare black basalt hills about the Homs Gap—the important pass, between the Lebanon to the south and the Jebel Ansariyeh to the north, through which runs the road from Tripoli to Homs; it looks out over a fertile and well-watered plain to the east. A position of great strategic possibilities, it was occupied even before the Crusades by a Kurdish garrison, from which it took its earlier name of Hosn el Akrad—Stronghold of the Kurds (2). This garrison was ejected by Raymond de Saint-Gilles in 1099, but the castle was only finally occupied by the Christians in 1110, when Tancred, Prince of Antioch, took it for the benefit of his ward Pons of Tripoli. In 1142 Raymond I, Count of Tripoli, gave it to the Knights Hospitallers, in whose hands it remained until its final loss in 1271. They made it the centre of their great strategic position, the network of castles and towers covering Tripoli from attack from the east. They also entirely remodelled it. The castle which we see to-day is a concentric fortress, one, that is to say, in which there are two lines of defence, the inner—which at the Krak is of a highly original and extremely formidable character—being built close up to the outer, which is overlooked and commanded by it. The enclosure of this inner enceinte is a great mass of vaulted undercrofts, enclosing a dwarfed and constricted courtyard. The castle of the eleventh century was a very different structure; its remains, still incorporated in the present buildings, are those of a single-ward castle, covering approximately the same area as the existing inner enclosure. There was a gateway set between two square towers (H, FIG. 1); this still remains, as do the postern-tower P and the Chapel, whose projecting apse served as a tower; these defended it on the north and east; on the other two sides there were probably four square towers. The walls were lined inside with a continuous ring of vaulted buildings. The position of the castle was a strong one; on three sides the ground fell in headlong slopes; the eastern slope, up which the approach from the plain zigzagged,

¹ Le Crac des Chevaliers (Les Châteaux des Croisés en Terre Sainte) Paris, 1934.

² Hence the curiously pleonastic modern Arabic name Qalaat el Hosn (Stronghold Castle).

ANTIQUITY

began directly from the foot of the walls, but there was a distinct level space on the north and west between the walls and the scarp ; on the west at least this was partly occupied by a ditch. The south side was the weakest ; here the ground rose in front of the defences, not indeed steeply, but very perceptibly ; there was no doubt a formidable ditch on this side.

Strong though this castle was—and the great Saladin, at the height of his fortunes, had misliked its appearance too much to attempt its capture—it was not strong enough for the ambitious rôle in which the Hospitallers had cast it, and at some date close to the opening of the 13th century it was very thoroughly reconstructed. On the north and east they left the existing defences unaltered, but the weaker south and west fronts were remodelled, the small square towers being replaced by large round ones, and the whole line of defence encased in a vast inclined talus, out of whose slopes the great towers rise majestically ; carried out in first-class masonry, this defence is extremely beautiful, and also very strong indeed. On the south, where the defences were advanced considerably in front of the older line, the talus rises from the bottom of the original ditch, now represented by the great cistern, whose lofty counterscarp partly conceals the immense slope of the walls. On this side there are three enormous round towers—I, J and K, set close together, forming what is probably the finest medieval line of defence anywhere ; they are of immense height and solidity, and are linked by two high terraces, supported on two storeys of vaults ; these terraces are connected by a wide crenellated ledge, passing round the face of tower J ; on the western terrace there stands a large vaulted structure of post-Crusader workmanship, which seems to have replaced an older construction. The upper room of tower K is a pleasant apartment, having a large window with floral enrichment ; this was evidently where the constable of the castle, or the Grand Master on his visits to the Krak, would be accommodated. The two upper chambers of tower J are lighted respectively by one and two fair-sized windows of two lights with ornamented heads ; here, and in the upper chamber of tower I (whose windows have been robbed) we would naturally expect the quarters of the Knights to have been ; there were 60 of them in garrison in 1255 (3) and this would be quite adequate accommodation for them—ample if we include the basement of tower J, a rather cheerless, unlighted room, and the building between towers J and K. Certain precautions were taken to protect this set of quarters ; across the back of tower J and the terrace between it and tower I a gap about ten feet wide was left in the lower vaults (4), leaving a protective drop to ground-level, which Deschamps calls a *saut-de-loup* ; this gap was crossed by a bridge—possibly a movable one—at the head of which there seems to have been a door. These good quarters and the measure of protection given them emboldened M. Enlart to allude to the three great towers as the *Donjon* of the place, and Deschamps repeats this description without apparent question (5), despite Rey's warning that the strong defensive works used by the Hospitallers are not to be confused with the European *donjon* or keep (6)—that is to say, are not the normal strong work, capable of independent defence after the rest of the castle had been captured. A little reflection will show how profoundly correct Rey's statement is for the Krak : none of the three great towers—much less the three together—can be regarded as independently defensible ; towers I and K have no stairs, and there is no communication between their floors except by way of the outside world ; on the

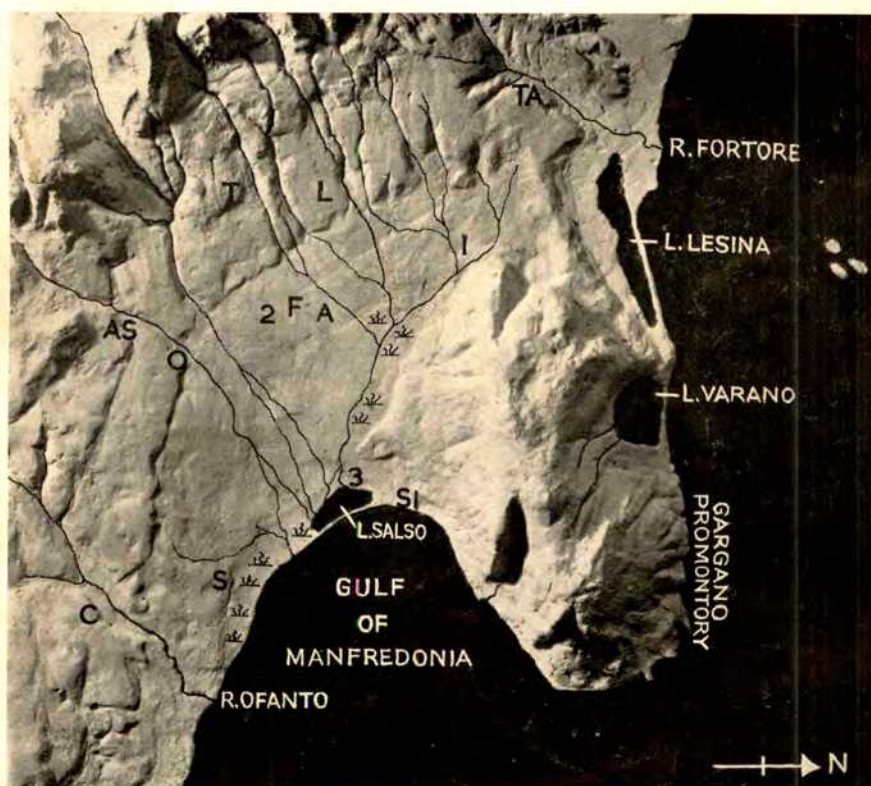
³ Bull of Alexander IV, quoted Deschamps, p. 130.

⁴ Its position on the plan is marked by almost the only Mamluk work in the inner ward, the Muslim owners having closed the gap.

⁵ *op. cit.*, p. 205.

⁶ *Architecture Militaire des Croisés*, p. 15.

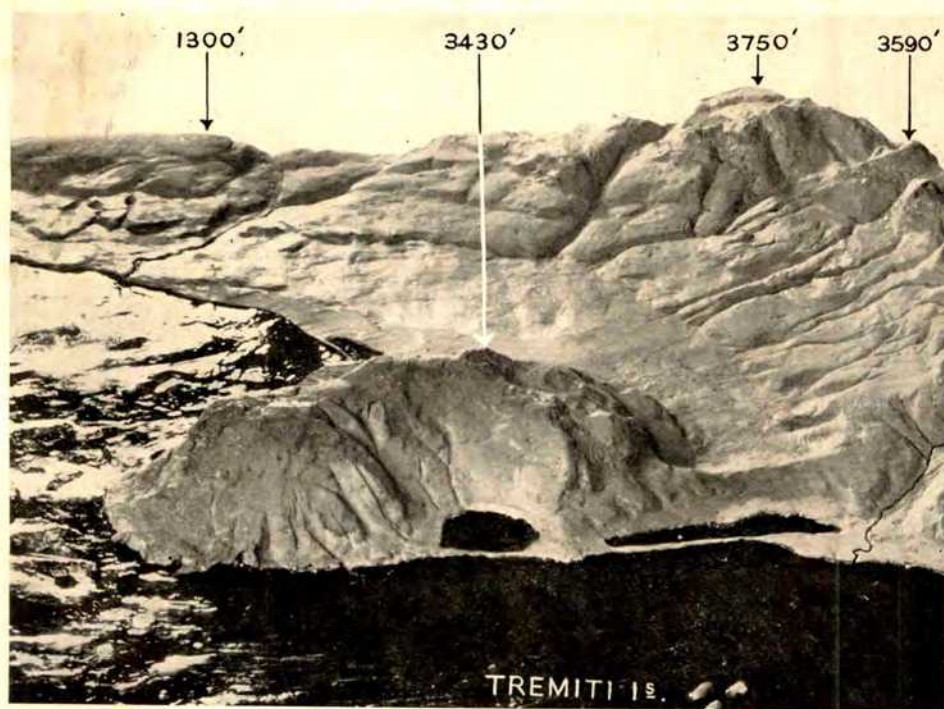
PLATE I



a. VERTICAL PHOTO. OF RELIEF-MODEL OF PLAIN OF N. APULIA, BETWEEN THE APENNINES AND THE GARGANO PROMONTORY

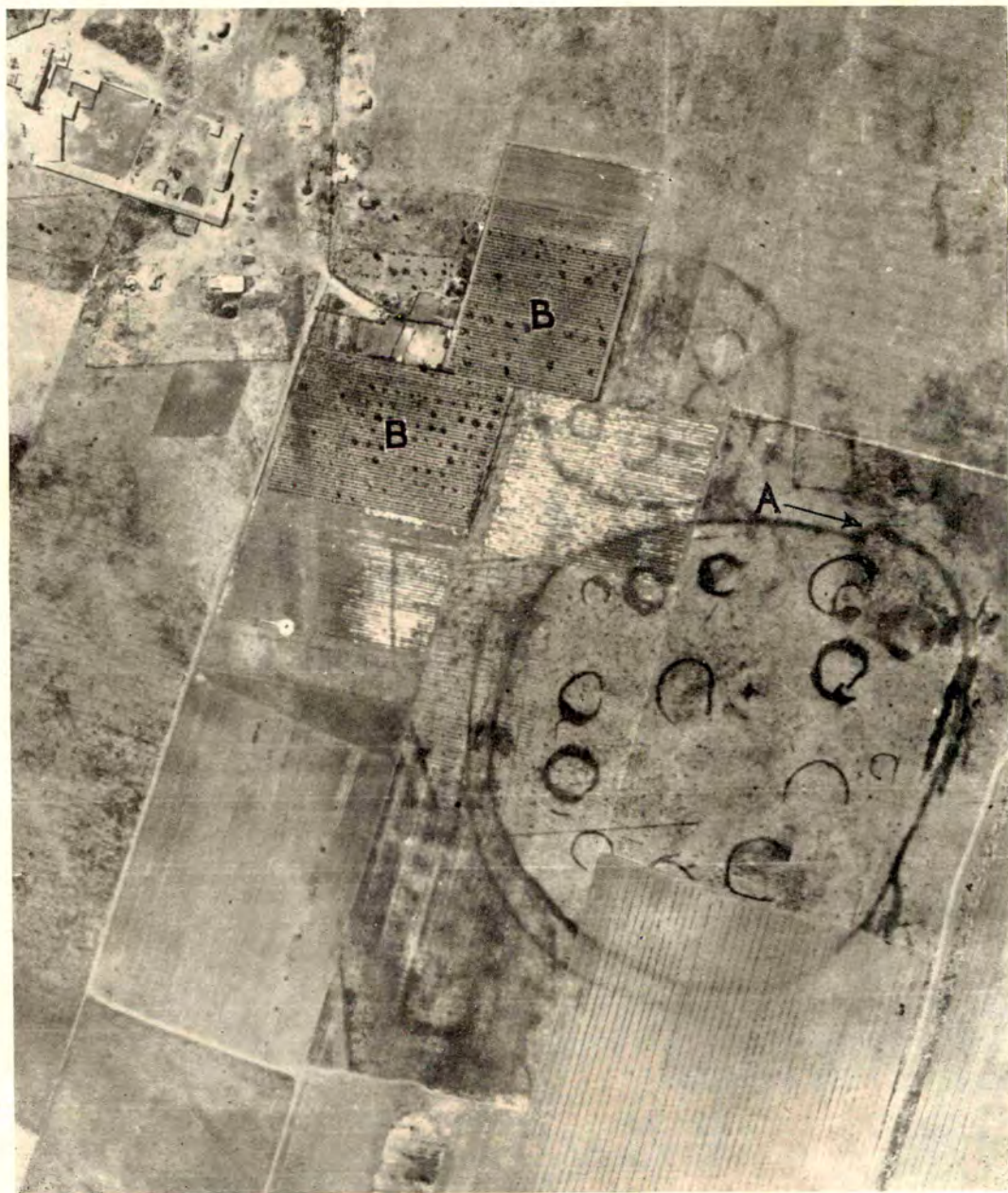
Scale about 1 in. to 16 miles

A=Arpi : AS=Ascoli Satriano (Ausculum) : C=Canosa (Canusium) : F=Foggia : L=Lucera (Luceria) : O=Ortona (Herdonia) : S=Salpi (Salapia) : SI=Sipontum : T=Troja (Aecae) : TA=Teanum Apulum
1=Plates IV and V : 2=Plate VI : 3=Coppa Navigata



b. OBLIQUE PHOTO. OF SAME, LOOKING S. OVER THE PLAIN AND DOWN THE COAST TOWARDS BARI, WITH SPOT-HEIGHTS ON THE MURGE (1300 ft.), THE APENNINES (3750 ft.) AND THE GARGANO (3430 ft.)

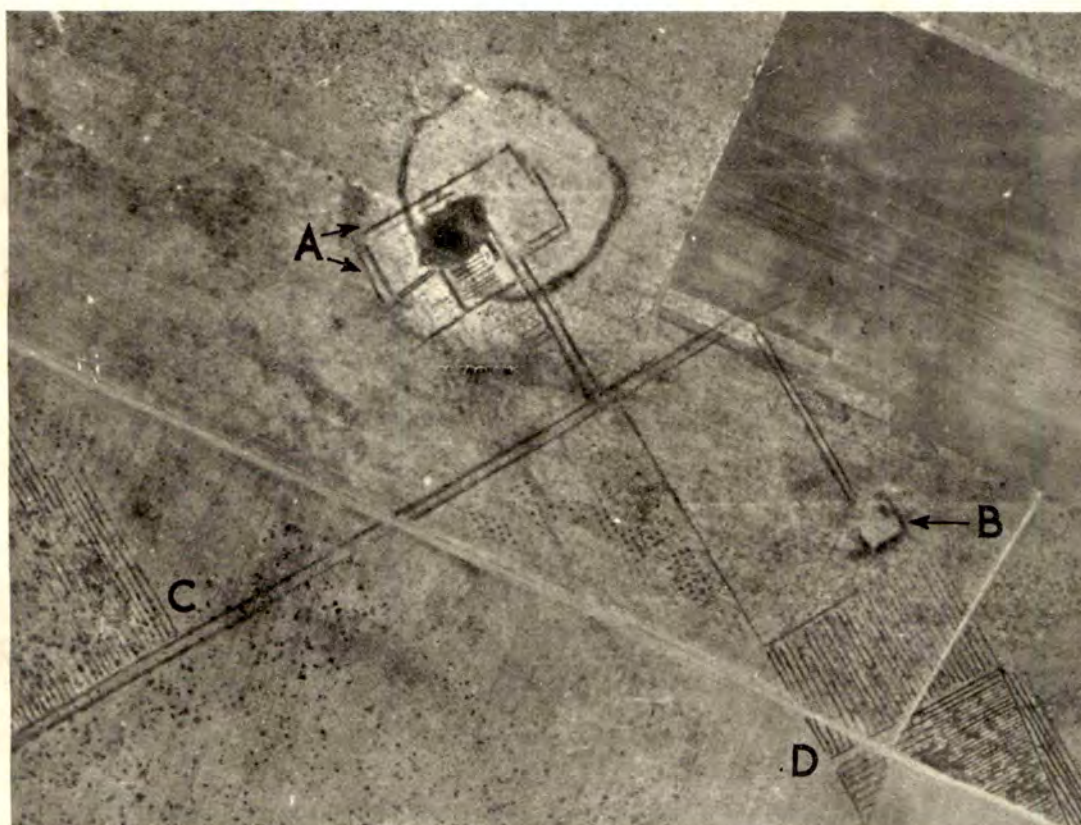
PLATE II



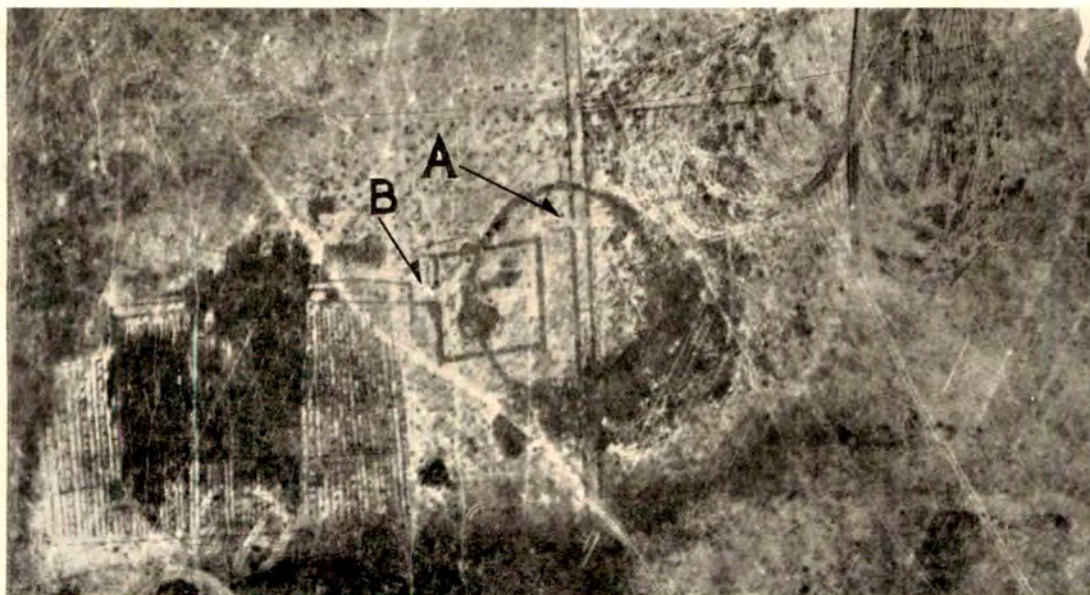
A VILLAGE (240 YARDS ACROSS) WITH 14 HUT-ENCLOSURES OF NEOLITHIC TYPE; REVEALED BY
CROP-MARKS $3\frac{1}{2}$ MILES N.W. OF LUCERA

Scale 1 : 3500

PLATE III



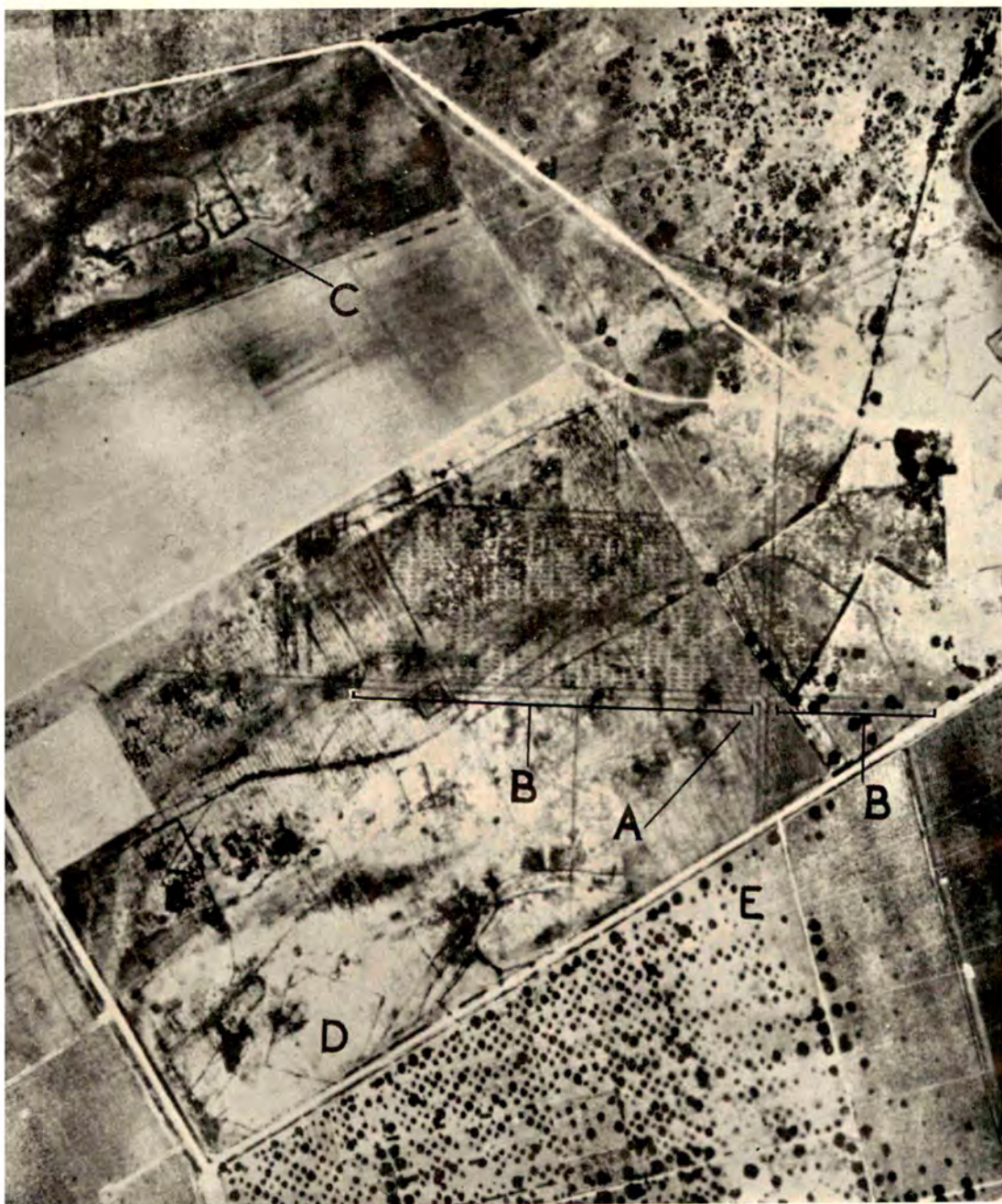
A



B

THE UPPER PHOTO, *a* (Scale 1 : 3500) AND THE LOWER *b* (Scale 1 : 3000) BOTH SHOW RECTANGULAR ROMAN FARM ENCLOSURES N.E. OF LUCERA, WITH ADJACENT VINEYARD PATTERNS, CONNECTED TO CENTURIATED ROADS. THEY ARE SUPERIMPOSED ON EARLIER CIRCULAR ENCLOSURES. ALL ARE REVEALED BY GRASS-MARKS AND CROP-MARKS

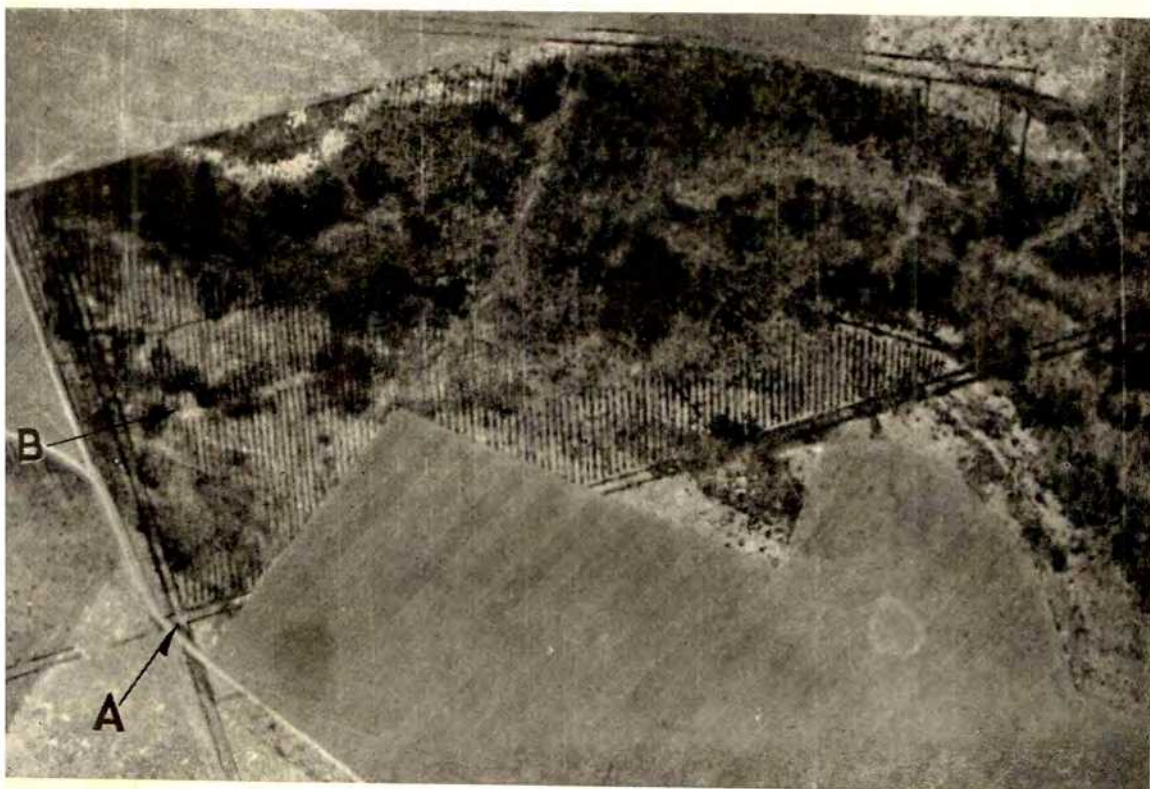
PLATE IV



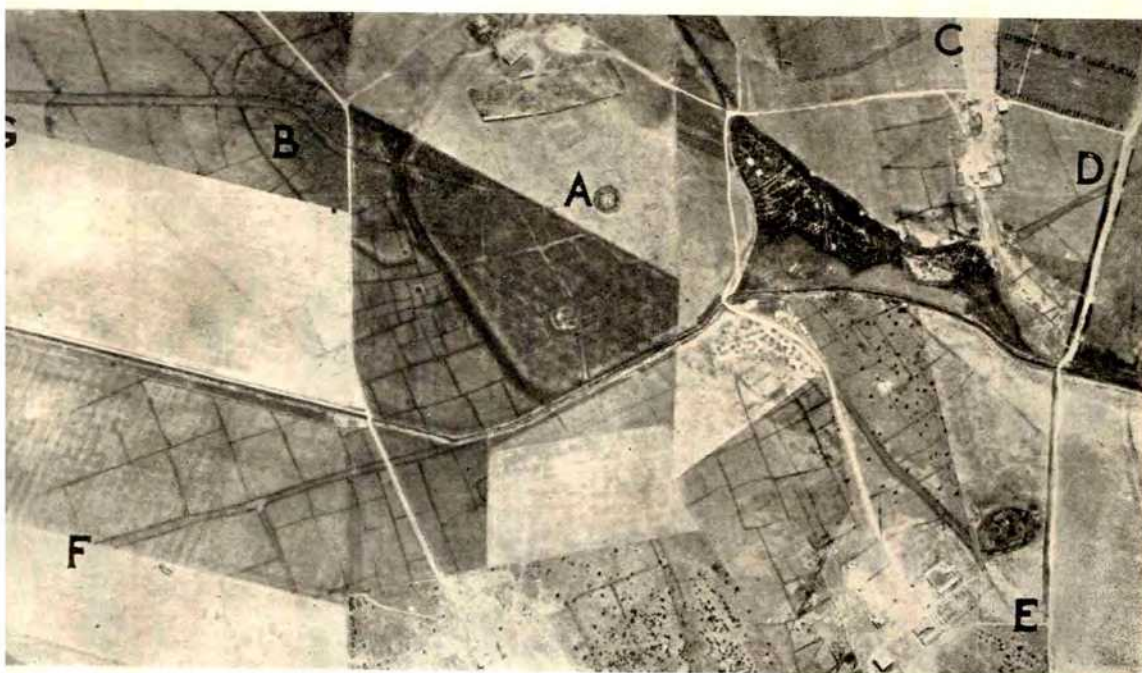
CROP-MARKS NEAR CASA SCOPPA

A=intersection of two centuriated Roman roads (*limites*).
B=the pattern made by cultivation-pits in a Roman vineyard or orchard.
E=a modern olive-grove for comparison.
C, D=other crop-marks.

PLATE V



A



B

THE INTERSECTION OF TWO CENTURIATED ROMAN ROADS (A), AND THE PARALLEL TRENCHES OF A ROMAN VINEYARD WITH A PATH THROUGH IT (B), ALL SHOWING AS CROP-MARKS; 4 MILES S. OF FOGGIA. Scale 1 : 3500.
A LARGE EARTHWORK (A) OF MEDIEVAL TYPE AT TORRIONE DEL CASONE, 13 MILES N.N.W. OF FOGGIA. THE OUTER HORNWORK (B) AND THE TRACKWAYS (C to G) WITH SMALL FIELDS BETWEEN THEM APPEAR AS CROP-MARKS

PLATE VI



A PROBABLE *PALACIUM* OF THE EMPEROR FREDERICK II AT SAN LORENZO, 3½ MILES S. OF FOGGIA; REPRESENTED BY A MASSIVE EARTHWORK OF MEDIEVAL TYPE, WITH 3 ENCLOSURES (A, B, C) WITH TRACES OF ROADS APPROACHING THEM (D to G), AND FIELDS BETWEEN THEM. X-Y AND Z ARE MODERN FEATURES

Scale about 1 : 8200

ANTIQUITY

lower levels these floors are quite defenceless (v. plan, especially for tower I); tower J is at least a self-contained structure, but its defensive possibilities were slight; with two unfortified doors, both perfectly accessible, three large windows overlooking the courtyard, and not a single loophole directed towards the inside of the ward; standing between tower K, which is its own height, and tower I, which is very little lower, and with covered access to its base on both sides available to the enemy, it could offer little resistance once the rest of the ward had fallen.

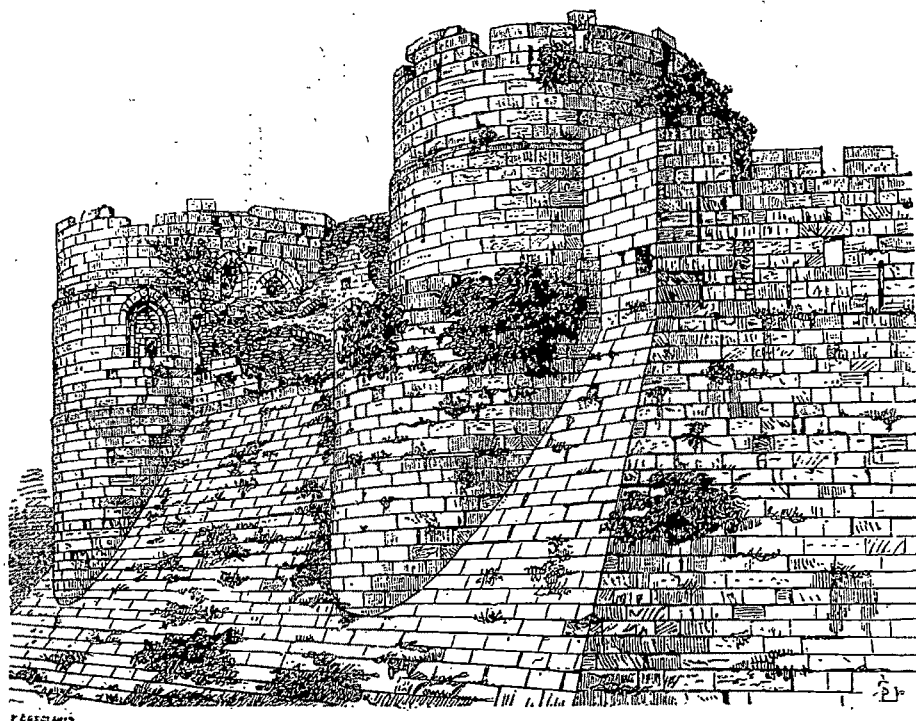


FIG. 2. LE KRAK DES CHEVALIERS: SOUTH FACE OF THE INNER WARD; TOWERS I (in background) AND J, SHOWING THE SENTRY-WALK ACROSS THE FRONT OF THE LATTER
(Since the date of this drawing, the parapets have suffered extensive damage)

It may be asked at this point what was the purpose of the elaborate protection of the *saut-de-loup*; and it appears to me quite certain that the significance of this *area* (in the 19th century domestic sense) was not strictly defensive; it existed for reasons of internal security. The garrison of the Krak included some dubious elements, particularly among the Turcoples, or mercenary light-horse; and while nothing in the nature of a mutiny *en masse* of the garrison was to be expected, there was always a danger of a *coup de main*, probably taking the form of a night attack and the assassination of the constable or the Grand Master in bed. Viewed in this light, the arrangements are excellent. The 'area' could be crossed easily with a ladder, but the mere possession of a ladder after dark would arouse suspicions, and it would be impossible to reach the terrace without attracting the notice of a sentry. In the same light, other arrangements of no real defensive significance are seen to have placed very grave obstacles in the way of treachery: the absence of stairs in two of the towers, and their disposition in tower J so that to reach the terrace it was necessary to pass through each room in turn, beginning with the less

THE TAKING OF LE KRAK DES CHEVALIERS IN 1271

comfortable rooms, which inevitably would be occupied by the junior knights—unrewarding, and most dangerous, subjects for assassination; finally, the arrangement of the great set-off on the face of tower J—round which lay the only access to the commander's quarters in tower K—with a door at each end so as to form a sentry-walk: all these precautions, useless against an enemy in overwhelming force, effectively disarmed internal treason. It has been found necessary to insist here on the fact that the great towers formed no sort of last refuge in a formal defence for the better explanation of the siege, as will appear below.

At the same time as the inner defences were remodelled, an exterior line of defence was added, carrying the perimeter of the fortress nearer to the top of the escarpment on the north and west; on the east the new line was inevitably carried down the slope, where it enclosed a limb of the zig-zag approach; on the south side it was advanced uphill on to a more unfavourable position, the ground sloping upwards rather sharply, so that the counterscarp of the ditch is almost level with the rampart-walk of the ward (7). This ditch, which is cut in the rock, and is about 30 feet deep on the uphill side, is a most formidable obstacle.

Later in the 13th century the castle was further reconstructed; on the east a series of great vaulted buildings was erected, and the line of defence further advanced down the slope, enclosing a second limb of the approach, so as to form in its entirety the famous main entrance, the Great Ramp. Beginning at a gate in the face of tower II, it forms a dark vaulted tunnel about 150 yards in length, with open sections at intervals, and a hairpin corner in the middle; no more unappetising entrance could be offered to an assailant, for it looks the sort of place where death might lurk at every step. Nor is this appearance delusive; the outer gate in tower II is weak, and the adjoining curtain is so thin that it appears that this part of the defence was a mere barbican; but the assailant would soon come up against a serious obstacle—the formidable double gate behind tower IC, the entrance to the outer ward. Beyond here the way divides, one road curving to the right and entering the open space between the lines of defence, the other continuing upwards to the elbow of the ramp. Here a strong gate (M) defends the ramp against attack from the south; at the elbow itself are the jambs of another gate, and the remains of a number of auxiliary defences of a perverse ingenuity. Behind this the ramp led up to the inner gate H and its fore-tower G. There is no gate in the fore-tower, nor do I think there ever was one, even before the rebuilding of this structure by the Mamluks. The gate H, however, was strongly fortified, with doors, a portcullis, and a large open machicolation in front. The whole length of the ramp is lined with openings in the crown of the vault; no doubt these served to admit light and air, but the careful tactical siting of some of them shows only too well that they were meant to let through more solid things than light.

To this later period belong the other outer gates, of which the small, well-defended entrance between towers 12 and 13 is dated to a time close to the middle of the 13th century by the inscription "*Au tens de Fre(re) Niciole Lorne fu fete ceste barbacane*"—'this ba-bican was built in the time of Brother Nicholas Lorgne' (8). On the south there is a gate in the flank of tower 8—one of the original towers of the outer line—which Deschamps considers to be an insertion (convincingly, because its curious and inconvenient arrangement, with the gates in the flank and the portcullis in the gorge of the

⁷ The parapet will have been of two storeys, in the Eastern fashion; this must have given a reasonable command over the counterscarp.

⁸ Constable of Margat, 1250-4, and of the Krak presumably soon afterwards.

ANTIQUITY

tower (9), could scarcely have emanated from the immensely competent architects of this line) and a short distance to the east of it a postern, opening in the curious salient between towers 8 and 9. Both these entrances were no doubt built to give access to the big triangular outwork on the south, which was evidently constructed about this time, to judge by the amount of black basalt rubble used in the construction of the later part of the defences; this evidently came from its rock-cut ditches. That the builders planned to give this outwork walls and towers seems probable; what defences it actually possessed at the time of the siege cannot at present be ascertained. Rey (10) suggests that it was palisaded, but timber is scarce locally; even in treeless Syria the Homs Gap is an exceptionally treeless region. It may have been given some provisional defence in the form of dry walling or light masonry. There is certainly no trace of any such work now, but the silted and encumbered condition of the two strong rock-cut lateral ditches suggests that it may have been cast down the scarp. It seems unlikely that the Hospitallers neglected to defend such an advantageously sited outwork, even though its walls may not have been sufficiently advanced to warrant a prolonged defence.

Before leaving the triangular outwork there are two last questions to be considered, both involving gradients. In the first place, the pronounced northward slope of the ground is masked by the high counterscarp and the outer curtain behind it, so that the full strength of the inner defences is concealed from view. The second fact is the manner in which the side ditches come into the main cross-ditch in front of the outer ward; for whereas the western ditch debouches on level ground, the eastern side of the castle has been twice enlarged downhill, as has been mentioned already, and as a result the eastern ditch opens on a steep slope.

Finally it appears that the Krak possessed a substantial walled suburb or *burgus*, (11) of which there appear to be no remains whatever.

Coming now to the narrative of the siege, I propose to examine the evidence, which is principally based on three Arab writers: Ibn Shaddad, Ibn Furat and Nuwairi (12); of these, Ibn Shaddad was a contemporary, but not an eye-witness, having joined the army three weeks after the siege; the other two wrote half-a-century later. For this reason, the account of Ibn Shaddad will be used here as the controlling narrative, and his dates will be accepted for the purpose of this article, though I cannot pretend to imagine, in face of the feats of the national Press during the recent war, that there is of necessity any advantage in accepting a contemporary account. The stages of the siege are narrated as follows:

(A) 3rd March 1271. Ibn Shaddad describes the Sultan as camping before the Krak and setting his mangonels in battery on this date: 'Now the castle had three lines of walls and three *bashuriyas* (a term whose meaning will be dealt with later). He pressed on the siege to the destruction of the ramparts on the 24th of the same month (13) (8th March).

Ibn Furat makes the siege open on the 21st February, and Marino Sanudo and the Christian authorities generally on the 18th.

⁹ So that the fighting-stage in the basement would be closed when the portcullis was down, and that on the upper floor when it was up!

¹⁰ op. cit., p. 46.

¹¹ Deschamps op. cit., p. 120; Rey, op. cit., p. 40.

¹² The excerpts from these writers are taken from the French versions quoted at length by Rey (*Arch. Mil.*, pp. 65-7), and Deschamps (*Le Crac des Chevaliers*, pp. 132-4).

¹³ The Moslem month of Rejeb.

THE TAKING OF LE KRAK DES CHEVALIERS IN 1271

(B) *5th March*. Ibn Shaddad: 'One of the *bashuriyas* was taken'.

Ibn Furat: 'On the 20th (13) (4th March) the "faubourgs" of the castle were taken'. Nuwairi says the same.

Now follows a delay in the siege; Ibn Furat and Nuwairi merely mention that the engines were being set up, but Ibn Shaddad says: 'operations were held up by incessant rains'. It appears that the siege was not pressed during this period.

(C) *21st March*. (14) Ibn Shaddad: 'The second *bashuriya*, called Haddadiya or Blacksmith's, was taken'.

Ibn Furat: 'The *bashuriya* was taken by storm. A place was made for the Sultan to shoot from'.

(D) *29th March*. Ibn Shaddad: '(the third *bashuriya*) was taken by the miners of Malik Sa'id . . . Then the troops stormed their way into the castle, massacred the Hospitallers, took prisoner the mountaineers, but let the villagers go, to keep up cultivation in the country'.

Ibn Furat: 'On the 16th of Shaban (30th March) one of the towers was broken; the Mussulmans made an attack, got into the castle and took it. The Franks fell back upon "the summit of the hill or of the castle (15)" . . . Engines were brought up into the fortress and trained on the hill'.

(E) *8th April 1271*. The Krak surrendered on this date, the garrison obtaining terms under which their lives and liberty were spared, and they were given a safe-conduct to Tripoli. Ibn Furat and Nuwairi mention that the Sultan sent in a forged letter, purporting to come from the Grand Master at Tripoli, giving permission for the garrison to surrender. Apart from the statement by Ibn Furat, quoted above, that mangonels were brought up, there seems to be no mention of any siege activity in these last ten days.

Van Berchem, and Deschamps after him, visualize an attack directed from the east, where the slope is said to be less formidable than on the north and west. The allusion to the Castle as having three lines of walls is said to support this. Van Berchem's interpretation of the chroniclers is an attack up the Ramp, beginning with a breach in the curtain 10-11, and ending in the defence being driven into the three great towers. As regards the breach, Deschamps shows that the curtain 10-11 was only refaced by the Arabs and not rebuilt (16). His own idea of the progress of the siege is more cautious, and is worth quoting in full:

'Ibn Shaddad, who gives the closest account of the siege, writes: "the castle had three lines of wall and three *bashuras* (barbicans)". From the fact that the eyewitness from whom Ibn Shaddad no doubt obtained this information saw three walls, it can be inferred that the main attack was made against the East front, that is to say, from the side of the bent ramp, whose walls could make up, as it were, three ramparts.

'The *bashura* or barbican taken on the 5th of March—which Sobernheim translates as "tower of entry", must be either tower 11, or the barbican 12-13.

¹³ The Moslem month of Rejeb.

¹⁴ This appears to be the correct date, but Deschamps (p. 133) for some unaccountable reason takes the 15th for the date, on the apparent authority of Ibn Shaddad, though the latter in fact gives the 21st.

¹⁵ This expression, which occurs in Rey, I take to be a translation of the word *qulla*.

¹⁶ p. 303.

ANTIQUITY

The second barbican taken on the 15th (17) (the tower of entry of the second enceinte according to Sobernheim) could be the gate at the entrance to the second element of the Ramp, the work G, or the postern of the tower P.

'Finally, the third barbican taken on the 29th March, in the attack on which the miners of Malik as Saïd took part, will be the set of defences placed at the opening of the ramp on the courtyard (the tower H).

'Then the Moslem soldiers stormed into the courtyard, where a certain number of knights were killed fighting. The last defenders took refuge in the great towers of the south . . . Then it is the 8th April, the surrender'.

Now any interpretation of the accounts of the siege must depend to a great extent on the translation of two Arabic words: *qulla*, a pleasantly vague word, meaning a tower, a peak, a small castle, or even a very large one (18); and *bashura* or *bashuriya*, which historians mostly translate as 'gateway' or 'barbican' in the narrative of this siege. Rey, however, makes it mean an outwork or advanced line of defence, and this is the translation given to it in other contexts (19). In fact, the most exact interpretation of the word is 'a bent entrance' (e.g. tower 8) (20), and 'barbican', in the loose medieval sense in which Brother Nicholas Lorgne's contemporaries used it, is a reasonable enough rendering. Whether Ibn Shaddad used it precisely, and if so, whether he knew exactly what he was talking about, is another matter altogether, and it might prove an unprofitable labour to attempt a reconstruction of the siege so as to involve the capture of three gates and three only, preferably with bent entrances, especially when we recall that it is quite possible that the gates of the *burgus* and of the triangular outwork may be included in the three *bashuriyaks* mentioned. It may, however, be worth pointing out that a forcing of the Great Ramp cannot be reconciled with this description; there were at least four gates, one of them double, in the way of this line of attack.

There are other drawbacks to the theory that the besiegers forced the Ramp: it seems curious that Bybars should have attacked on the east, in many ways the strongest side of the fortress; but the mere difficulty of the operation, and the *obvious* difficulty of it—the very appearance of the Ramp is alarming—make it unlikely that any assailant would willingly attempt it. Nor am I much impressed by the argument that the attack must have come from the East, because Ibn Shaddad mentions three lines of walls, and on this side the fortress presents the illusory appearance of having three lines; in fact it merely appears to be a jumble of towers; if on the other hand there was so much as a breastwork round the triangular outwork the castle would certainly have three lines of walls when viewed from the *south*.

But this is not the weakest part of Deschamps' and Van Berchem's case. Let us consider the three stages of the development of Bybars' attack, stages B, C and D above, corresponding to the taking of Ibn Shaddad's three *bashuriyaks*: Stage B was reached almost at once—only two days after the opening of the siege—and is strongly suspect of involving only a dependency of the castle, probably the *burgus*; on Stage C we have less information, though Ibn Furat's allusion to a place being made for the Sultan to shoot from seems to suggest that the position which had just been taken was a fairly commanding one. But we have one most important piece of information about Stage D: it was achieved by mining; Ibn Shaddad mentions miners, and Ibn Fūrat states that a tower

¹⁷ Vide supra, note on this date.

¹⁸ The gate of the Citadel of Cairo is called Bab Qulla.

¹⁹ Deschamps, p. 120; Rey, op. cit., p. 151.

²⁰ I am indebted to Professor K. A. C. Creswell for this fact.

was broken. We can deduce other facts: Stage D involved the fall of a major work, not just that of a single tower, for Ibn Furat calls it 'the castle', and Ibn Shaddad mentions a heavy slaughter and the taking of prisoners, including the local villagers. It would be tempting to follow Deschamps and see in this the capture of the inner ward, thus identifying the *qulla*, into which the Knights retreated, with the three great towers. But this interpretation at once raises difficulties, for whatever the *qulla* was, it was obviously a very strong position. The Knights held out there for ten days, and only surrendered on favourable terms, and after receiving a forged permission to yield. It may be objected that a good deal of this time may have passed in negotiations, but it is obvious that only a strongly-placed defence would be allowed to waste the besiegers' time and obtain good terms at the end of it. The Circassian Mamluk Bybars was a fine fighter—bold, resolute and resourceful, but treacherous, cruel and fanatical; such a man would hardly grant an extended parley and generous terms to a garrison who were at his mercy; and if the *qulla* is to be identified with the three great towers, they would certainly be at his mercy, for as we have seen, they would have had no adequate defence.

Moreover, if the inner ward fell, if fell by mining; where then are the traces of the mine? The excellent plans of M. François Anus, on which the plan to this article is based, show no 'Arab' work in the inner ward at ground level except a very few interior features—partition walls and the like. The fore-tower G has been rebuilt, but even if this contained a gate, its fall would not have involved that of the inner ward, nor of any place sufficiently large to accommodate the number of people that Ibn Shaddad mentions. In short, it is clear that Stage D was the fall of the *outer* line, not the inner, and that the *qulla* is the inner ward.

We have now progressed some distance, but there remains a final question to be settled: where the mine was sprung. Now the outer ward, unlike the inner, has undergone a great deal of reconstruction by its Mohammedan owners. Much of this consisted of refacing, but there are a number of points at which the whole wall is the work of the Mamluk owners: towers 6, 7, 9, 10, the face of 12 and a length of the curtain adjoining, and the curved portion of 13. Of these six, three can be dismissed at once: tower 7, an immense square tower built by Sultan Qalawun (21) stands in front of a Crusader curtain-wall which has been refaced, but is otherwise complete; tower 10 occupies an undisturbed Crusader plinth, and tower 13 is still very much as its Christian builders left it; the Saracen work consists merely in a bow-front applied to the tower to change it from the rectangular to a form approximating to the round. A similar change was made in the companion tower 12, but here the alteration was more extensive, involving a considerable piece of the curtain. This is, however, somewhat reduced in length if one considers the Crusader structures remaining intact behind it, and the total width of the breach in the Frankish work is no more than 40 feet. The narrow and rather ineffective tower 9 is even less likely; for one thing it seems to have had no Crusader predecessor, for another its whole width, from the battered plinth of the great vaulted building on the north, which partly underlies it, to the curtain between it and the southeast angle, is only 36 feet; this is a perfectly possible figure for the bottom of a breach, but the gap is little or no wider at the top of the very lofty southern curtain, which could hardly be the case if there had been a violent structural collapse. Evidently the curtain was carefully cut back by the builders to make room for the tower.

This leaves tower 6, and here the gap is wide—fully 75 feet across—and includes not only the tower, but a short length of the adjoining curtains on each side. Finally, it

²¹ A very massive structure, its base being a solid cube of masonry of side approximately 50 feet.

ANTIQUITY

stands at the most readily accessible point of the whole enceinte. It must be remembered that mining operations in the Middle Ages were very seldom conducted by means of a long subterranean gallery, as they were in later periods; nor is it likely that a mine of this exceptional type would be attempted in the hard volcanic rock underlying the Krak. If it were attempted, it is improbable that it could have been brought to success in the very limited time of the siege in question here. The normal practice was to work at the very foot of the wall under cover of the strong movable shed variously called a 'cat' or a 'sow'. As the natural reaction of the defence would be to drop upon this engine all the heaviest and most harmful objects they could collect, its construction had to be massive, and hence its weight was considerable; and this weight had to be man-handled to the foot of the walls. It would have been almost impossible to do this up the violent slopes on three sides of the Krak, while most of the south side is covered by the great ditch, whose counterscarp would be the principal obstacle, being more or less vertical, and 30 feet high. To get a heavy 'cat' down this, even without the attentions of the besieged, would be a tricky and dangerous business. It followed that the best points of attack would be the southeast and southwest angles of the castle, by the way of the lateral ditches of the outwork. The eastern ditch opened on a sharp slope, as has been mentioned; the western on fairly level ground opposite a tower—a more vulnerable point than the curious salient on the east; this, then, must be the position of Bybars' mine.

We can now proceed to give an amended account of the siege and capture of Le Krak des Chevaliers: Bybars himself appeared before the fortress on the 3rd of March 1271; to judge by the earlier dates mentioned by some of the chroniclers, it may have been under observation, or even blockade, for some days previous to this. Two days later the *burgus* was taken, and on the 21st, after sixteen days of rain and hard work, setting up the mangonels on the plateau to the south of the castle, the Sultan's men rushed the triangular outwork, from a position on which Bybars was able to follow his old army trade of crossbowman. Next, the outer ward was attacked, and the southwestern tower was mined and collapsed on the 29th. Then the Moslems stormed the outer ward, which was full of the local peasantry (a practice recommended by Procopius), and slew the few members of the garrison there; these last were doubtless *enfants perdus*, with the gates shut at their backs. And now the victors found themselves facing the inner ward across a swimming gulf; the northward slope of the ground has several times been mentioned in the course of this article, together with its effect of making the three great towers of the second line crouch low behind the defences in front of them, as if to conceal how frightful they really are. But now the whole awe-inspiring spectacle was revealed. It may be said here that Deschamps' photographs, excellent though they are, are quite inadequate to convey the full impression of this incredible line of defence; it is not, in fact, a possible subject for a camera.

The Sultan was now in the dismal position of one who has forced three lines of defence, and finds the fourth impregnable.

But al-Malik az-Zahir Rukn ad-Dunya wad-Din Abu'l-Fath Bybars had not risen from a slave-soldier to a victorious Sultan for nothing; reflecting that the Knights were probably even more downhearted than himself, he tried his luck with a forged letter, in the reasonable hope that they would be unwilling to scrutinize it too closely. His resource and treachery were rewarded; the Knights asked for quarter, and were granted it; and on the 8th day of April 1271, the 'key of Christendom' passed into the hands of Sultan Bybars. Not till the war of 1914-18 was it to be in Christian hands again.

Kingship and the Gods: a review*

by M. E. L. MALLOWAN

EVERY schoolboy knows that King Rimush of Agade was struck down in a Palace revolt—some say pierced to death by sharp bodkins, others that his skull was smashed by heavy stone tablets. (1) And it is now almost common knowledge that Bur-Sin died of a pinched shoe—it may have been following on a septic foot contracted at Eridu, when he was pacing the sand in the precincts of his unfinished Ziggurat. No less undignified was the death of Sin-iddinam of Larsa, crushed to death in his Palace by the fall of a staircase. But who ever heard of a Pharaoh so ludicrously stricken? (2) There lies the difference between the Egyptian and Mesopotamian concept of monarchy. The Pharaoh was a god and the son of a god, made in the image and likeness of Horus, son of Osiris: it is true that he was mortal, as was more than one of the ancient gods, but his mortality lay lightly upon him because when the time came for his end upon earth he was predestined to rebirth as Osiris. In the Tigris-Euphrates valley a king, whatever else he might be, was pre-eminently a man, even if only a little lower than the angels, and even the mighty Sargonid Kings of Assyria, could on occasion receive and accept a rebuke from their subjects. (3) Indeed the Assyrian kings, whom we are accustomed to think of as arrogant warriors, were no less the slaves of the society they served than the humblest members of their realm. From records of the first millennium B.C. we hear of an Assyrian ruler being bidden to commit on himself a painful act of depilation: on another occasion after fasting to the point of inanition he was made to persevere yet further by the relentless priest who was responsible for his abstinence; at other times he had to endure an enforced solitude, segregated like a leper in a reed hut—an act of contrition and purification on behalf of his realm. It is true that the Pharaoh's lot was often hardly any happier, for he too was hemmed in by every kind of *tabu*, and his engagement and duties book, if strictly followed, would have kept him busy for every moment of his pompous life. But at least the Pharaoh might enjoy the calm serenity that comes to those who believe that their translation to the stars is inevitable. In sum the Egyptians were optimists, for experience had taught them that the sun would not fail, that the river would rise again and that the harvests were generally good. Mesopotamian pessimism on the other hand may well have been induced by the stark uncertainty of their climate; by constant fear of invasion from the hills and the ever-recurring threats, natural and preternatural, to the fabric of their society. Perhaps the Egyptian strain of happiness owed something to a negroid admixture within their race, (4) and conversely

* *Kingship and the Gods*: A study of Ancient Near Eastern Religion as the Integration of Society and Nature, by Henri Frankfort, *The University of Chicago Press*, Chicago. 444 pp., 53 illustrations. Price 5 dollars.

¹ C. J. Gadd, *History and Monuments of Ur*, 96 ff. See also *Journal of Cuneiform Studies*, 1, vol. 3, 1947, for other evidence relating to the deaths of Rimush, Bur-Sin and Sin-iddinam.

² Some of the Pharaohs suffered violent deaths, e.g. Sekenenre of the xviiith Dynasty was assassinated; but there is nothing comparable in the Egyptian records to the facts quoted about these three Mesopotamian monarchs.

³ R. Labat, *Le Caractère Religieux de la Royauté Assyro-Babylonienne*, 23 f.

⁴ But a contrary opinion is held by Junker who does not believe that there was a negroid admixture. See *J.E.A.* 1921, pp. 121 ff., 'The First Appearance of Negroes in History'.

ANTIQUITY

Mesopotamian gloom may have been in part the outcome of the morose and suspicious temperament which is characteristic of the Semitic and other nomads.

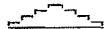
The theme chosen by Dr Frankfort for his study has proved to be an effective method of reanimating Egyptian and Mesopotamian religion for his readers. By focussing our interest on the king he has given us a sense of orientation in the strange and often unfamiliar realms of religious ideas. The king has turned out to be a kind of sheet-anchor alike for ancient society and for us ; he it was that gave perpetual coherence in the ordering of the state and in the tradition of religious practice. The contrast between Egypt and Mesopotamia which emerges from a study of their religious beliefs has been nicely re-emphasized by a brief epilogue on the Hebrews, whose prophets were fundamentally antipathetic to the more ancient concepts of monarchic rule.

A great variety of religious topics comes up for consideration in this book which deals in the first half with Egypt, in the second with Mesopotamia. When the curtain rises on the Egyptian stage, Menes is seen to be the hero of the first act : in him as the first representative of a united Lower and Upper Egypt we find that tendency towards Egyptian dualism which Frankfort constantly detects in Egyptian thought. Two thrones, two crowns, life and death, body and soul, the Ka as the animating principle of life, Osiris and Horus, the relation between the dead and the living monarch : all these concepts may be interpreted as evidence of a tendency to reflect on the Universe as a dualism of forces held in an unseen equilibrium. It is true that the Egyptians did not expressly formulate a dualistic philosophy such as that of Zoroastrianism ; but it is evident that they had an acute awareness of the unseen powers which were constantly at hand and mysteriously manifested to man. Thus the power of the ghosts of the king's ancestors was revealed in the guise of wolves or falcons—ghostly relics of the more widespread beliefs characteristic of the Old Stone Age : these spiritual origins have recently been vividly described in Miss Rachel Levy's book *The Gate of Horn*. Such phantasies and many others besides emerge from the shadows of the great series of Egyptian festivals which embodied the aesthetic perception of a balance between the functioning of man through the king, and the ordering of the cosmos through the gods. In the Coronation and Accession ceremonies, the Sed festival of the King's jubilee, the Transfiguration and the Mystery Play of the Succession, we find the scenic representation of Egyptian belief, moving and passionate, set in intention : yet at this distance we can but see them through a glass darkly. On all these subjects Dr Frankfort has clearly formulated his ideas and has never left us in any doubt about what he means. But much in the ancient texts is still obscure, much was left unsaid by the people ; much was thought and much more was felt : consequently we can hardly be expected to be convinced by all the conclusions of any one interpreter. For example : on the subject of Isis, Frankfort holds the view that in pre-Ptolemaic times the function of that goddess was to act as a kind of hypostasis for the king's throne⁵. I cannot see that the evidence favours his theory at all strongly and there to my mind his persuasion has failed. He has however drawn an effective contrast between Hathor and Isis. Hathor who dominates the topmost register of the Narmer palette was an exceedingly ancient goddess,—surely a common heritage of predynastic African and western Asiatic belief ; her function was to act as the divine mother and she was imagined as a cow ; she betokened the Pharaoh's divine right of succession, and in her capacity as a procreative goddess symbolized a long-held belief in the link between the reproductive powers in nature and

⁵ It is, however, true that her name means 'seat'. None the less, as I have argued below, I believe that her powers had a wider significance.

KINGSHIP AND THE GODS

in man. Isis, who incidentally was by no means always represented with a symbolic throne, was in Pharaonic times a comparatively neutral figure. Her presence was necessary because it was felt that Osiris had to be accompanied by a female counterpart—wife rather than mother, and as wife it followed that she was less obtrusive than the mother. For, as Frankfort has well emphasized, the Egyptian religion was essentially masculine. It was the king and Osiris who held authority over the living and the dead, a Sun-god and not a Sun-goddess who was supreme in the pantheon, and it is significant that when Hatshepsut, a queen, came to rule over Egypt, she was frequently represented in the guise of a man.

How delicate is the task of interpreting the complexities of Egyptian religious belief may be seen by comparing Frankfort's explanation of the Pyramid with that of I. E. S. Edwards, who has recently discussed this problem in his Pelican book on the Pyramids of Egypt. Frankfort accepts the thesis previously sketched by W. B. Kristensen and De Buck, that the Pyramid 'was the specific Heliopolitan form of the Primeval Hill, the Benben.' According to Frankfort, therefore, the Pyramid, while it was the Pharaoh's resting place, was also the symbol of the first land to emerge from that watery chaos which according to the Memphite theologians was the source of Ptah the Risen Land, the first evidence of creation. Such a concept incidentally was an intellectual account of Creation which ran counter to the popular belief in the Sun as the source of life. However that may be, it seems to me that I. E. S. Edwards (6) has given us a far more convincing account of the true significance of the Pyramid in relation to the Pharaoh. He has pointed out that the Pyramid is a pictorial representation in solid form of the rays of the sun shining down upon the earth; that the pyramid texts describe the king as ascending to heaven on the rays of the sun; and that *m(e)r*, the Egyptian word for a Pyramid, is capable of interpretation as a root meaning 'to ascend' prefixed by the letter 'm' conveying the meaning of place. The Pyramid would therefore mean the place of ascension, emphasizing thereby the means by which the Pharaoh in his solar aspect ascended to heaven. It would seem therefore that the true Pyramid was not the material replica of the Primeval Hill of Creation, but rather of the Sun's rays. Moreover the hieroglyph in the form of a double staircase  attached to the sense sign 'r' meaning ascent could well have been a stepped Pyramid.

When Frankfort turns from Egypt to Mesopotamia he is at pains to stress the difference between the two views of the Universe. And he expresses this by saying—and he says it repeatedly in more or less similar terms,—that the Egyptian 'could not conceive anything preceding the establishment of his static world'. His meaning is clear enough: it emphasizes the distinction between the Egyptian belief in a well balanced pattern of natural forces, and the Mesopotamian awareness that man and society were but precariously established on earth and wholly dependent on the whims of the capricious gods. But the use of the adjective 'static' to describe this Egyptian concept is not altogether happy, for static implies something immobile as well as in equilibrium. The only thing static about the ancient Egyptians were their massive monuments; their world was nervously mobile and everything was in a state of rotation, from the Sun in his bark crossing the sky by day and the underworld by night, to Osiris and the king-rainmaker or the seasonal fluctuation in the level of the Nile. The fact is that to the ancient Egyptians the world was a recurring succession of events, only pre-ordained in so far as a supreme effort was made by the divine king and the hierarchy to bring the right ordering of affairs to its expected conclusion. And when the outcome did not come up

⁶ I. E. S. Edwards, *The Pyramids of Egypt* (Pelican Books), 234 ff.

ANTIQUITY

to expectations then the Egyptians were not a little puzzled ; but like ourselves they came through dark periods by a resolute refusal to accept the facts, thereby for nearly three millennia avoiding the logical consequences of external pressure and alien ways of thought.

We have already noted at the beginning of this Review a point to which Frankfort has often drawn our attention, that the Mesopotamian king was always more human than divine ; aspire as he might to divinity, his feet were firmly planted on the ground, and he had no heavenly counterpart like the Egyptian Osiris, who was the dead king incarnate in heaven. In discussing the relationship of the Mesopotamian king to the gods Frankfort has drawn a sensitive sketch of the theological concepts, and he has shown how dangerous it is to accept the divine sign which sometimes preceded a king's name at its face value. It is true that at certain periods of Mesopotamian history there were actually cults of the king, for instance of Shulgi at Ur ; that Naram-Sin was represented as wearing the horns of divinity ; and that one passage in the cuneiform texts relates that Eannatum of Lagash was actually suckled by a goddess. But generally speaking it is clear that the expressions describing the divine parentage of Assyrian and other kings in the Tigris-Euphrates valley were figures of speech, drawing attention to the divine right of kings, rather than to physical kinship with the gods. Indeed Frankfort has acutely remarked that the Sumerians wrote that *kingship* descended from heaven : they did not say *kings*. The fact is that the divine nature of kingship has been evoked in many different places and at many different periods by monarchs and their political satellites, without implying that the king himself was divine : it was the Egyptians who were pre-eminent in stressing the actual divinity of the king's person as a physical manifestation of god.

Far more mysterious than the kingship were the divine Mesopotamian symbols ; statues, suns, moons, crown, sceptre and other regalia which, as Thureau Dangin has pointed out in the Akkadian texts, were thought of as endowed not only with life, but also with motion (7). There is a simple and touching humility in the worship of the Assyrian kings' mantle which in the absence of the king's person might be displayed in the cities of his realm to receive propitiatory prayers. How poignant a reminder that is of Christ's healing of the sick woman at Gennesaret who had but to touch the hem of his garment to be healed by the virtue that lay therein (8). That humble faith in the power within the garment of Christ the King must have been deeply rooted in a more ancient belief that the virtue of kingship rested in the king's mantle. Nor has the notion concerning the symbolic nature of the mantle yet left western Asia or Africa where the captains of pilgrim bands to Mecca, fountain head of Islam, are invested at a solemn ceremony with a gorgeous robe, the symbol of their holy mission.

In comparing the Mesopotamian with the Egyptian king it was inevitable that some discussion of their funeral rites should occur, for the disparity of practice between those two countries brings out as clearly as any other aspect of their religions the differences of concept concerning the king. The death of a Pharaoh was an event in which the whole of the Nile valley was vitally concerned, involving as it did the Succession in heaven and the dead king becoming Osiris, while on earth the living counterpart of Horus had to assume the kingship through a series of progressive ceremonies at which Egyptians from most distant parts of the country must have converged. By comparison our knowledge of what happened in Mesopotamia when a king died is meagre, though it may be inferred that Assyrian royal burials were attended with every show of pomp and circumstance ;

⁷ F. Thureau Dangin, *Rituel Accadiens*.

⁸ *Matthew* x, 20, 21 ; *xiv*, 34 ; *Luke* vi, 19.

KINGSHIP AND THE GODS

but the theological significance of the Succession, so prominent in Egypt, was relatively unimportant in the Mesopotamian ceremonies. The burial of an Assyrian king was, as we may gather from an Assyrian text, (9) an act of filial piety, and there was added a note of general sadness at which all nature mourned :

‘ The channels complain
and the watercourses respond :
Of trees and fruit
the face is darkened ’.

We know however from other texts to which Frankfort has not had occasion to refer that mysterious ceremonies did take place at royal burials in Assyria. E. Dhorme (10) has commented on an Assyrian tablet which relates that a lady of the Palace whose function was apparently to serve as the dead king's bride spent some time in the tomb before it was finally closed. One of the essential parts of the ceremony was the *taklimtu* or monstration of the body which had previously been hidden. The lady herself conducted a ritual of purification, washed and kissed the dead king's feet, passed three times round the bed and, after substituting an image in her place, was released from her duties. Parallel with that text there is a remarkable letter written in the reign of Esarrhaddon (11) describing how one Damqi and a lady of the court were sacrificed as a substitute for the king. Another text from Assur records the sacrifice of a virgin kid to Ereshkigal, queen of the shades, as substitute for a man. The animal was treated like a corpse,—embalmed, dressed, shod and adorned with eye-paint to give it the aspect of its substitute at the monstration. These texts and a few other scattered references (12) offer about all the textual light which we may glean if we would attempt to understand the nature of the so-called Royal Burials discovered at Ur and at Kish, and presumed to have occurred at Khafajah in the early dynastic period, about two millennia before the reign of Esarrhaddon. But they do lend colour to the view that the bodies in the shaft graves at Ur were not necessarily those of kings or Royal Princes—though whether they were substitute-burials or the bodies of persons who had participated in a fertility rite connected with the sacred marriage is still in dispute. Frankfort himself inclines to the theory that the death pits at Ur were substitutions for the kings. It is interesting to note that some of the regalia within these tombs, especially the golden pick-axes, were probably symbols closely connected with the god Enlil, associated in the early Sumerian literature with the Creation of mankind (13).

Within the limits of this book Frankfort has discussed a wide range of ancient beliefs ; he has marshalled his evidence skilfully and succeeded in making a connected unity out of the complex and perplexing variety of evidence which has come up for examination. Towards the end of the book he has a number of well chosen remarks to make about the ‘ suffering gods ’ of Mesopotamia ; and in an excursus on Tammuz, Adonis and Osiris he stresses the need to differentiate the peculiarly individualistic story-patterns associated with each of these divinities in Mesopotamia, Syria and Egypt, and calls attention to the need for avoiding facile assumptions about a common origin. Yet it is interesting to find that, when reviewing the part played by the wild boar as the common enemy of each of these gods, and after noticing the evidence for

⁹ Quoted by Frankfort on p. 244.

¹⁰ *Revue d'assyriologie et d'archéologie orientale*, 38, nos. 2-4.

¹¹ Quoted by Frankfort on p. 264.

¹² See also Sidney Smith in *J.R.A.S.* 1928, p. 849, f.

¹³ S. N. Kramer, *Sumerian Mythology*, 51 ff. on the creation of the pick-axe.

ANTIQUITY

a historical connexion between Osiris and Assur, he is forced, almost against his will, to recognize a continuous chain of thought in these three aspects of Mesopotamian, Egyptian and Syrian religion.

What exactly, we may ask, has Frankfort achieved in writing this book? I think it is this. He has succeeded in presenting to us both Egyptian and Mesopotamian religion in a coherent picture composed as an organic unity, with certain well defined and distinctive characteristics which differentiate them from any other of the important religious systems known to mankind. And he has achieved this by resolutely setting his face against considering the religion of these two countries as a gradual accretion of unconnected beliefs, whether acquired from a number of different localities or, as Kees would suppose of Egypt, gradually built up by a venal priesthood. He is not much concerned in showing the historical development: his purpose has been to show that from Early Dynastic times down to the advent of Hellenism each of these two religions had a perfectly distinct and recognisable character of its own, and that each was unified by certain basic concepts inherent in the mental and emotional make-up of the people. The same continuity which may be discerned in the arts, the economy, and the structure of society within the river valleys of the Nile, Tigris and Euphrates is found to apply to their religion too. In this respect he has managed to preserve a fine balance of mind, avoiding the temptation so freely indulged in by authorities such as Sethe and Kees to account for various aspects of religious belief by making hypothetical reconstructions of more ancient systems on the basis of the earliest known distributions of certain symbols. There has been no need to set the delta at loggerheads with upper Egypt. Frankfort has conversely avoided the temptation of considering each religion as conforming to some widespread ancient pattern of belief, and of forcing analogies from disparate material or cultures which have a unity and identity of their own. In this respect he criticizes the views of scholars such as Engnell, Hocart and Widegren. Yet he himself has at times been unable to avoid the methods of those whom he criticizes, because it is impossible to deny the interplay between one civilization and another. The transmission of religious thought has in the end had little regard for national, ethnic or geographical frontiers, and there remains some irreducible nucleus of human belief which would appear to have a common ancestry deeply rooted in the unforgotten past of prehistory.

This, in my opinion, is Frankfort's most mature work—rich in erudition, clear and balanced in presentation, with a discerning eye for certain basic concepts which give each religion its unity. His outlook is rational and free from prejudice, yet sensitive to the fine nuances of religious thought. The result is a tribute to his perceptive industry and to his capacity for absorbing polyglot learning and yet retaining his own originality of mind and individual method of approach. Not the least valuable parts of his work are the footnotes—that happy refuge of the scholar, often the repository of his best thinking, politely banished to the end of the book so as not to offend the untutored eye. Here we have a mine of references doubly valuable on account of the timely and enlightening comment which they contain.

The study of kingship in Egypt and western Asia is a profoundly moving story for all who are interested in the spiritual and mental development of man. Here we may follow the course of human striving and human suffering and perceive the mood of ancient man in his anxieties and his exaltation giving expression to the entire range of the human emotions:

Sunt lachrimae rerum et mentem mortalia tangunt.

The tale of ancient religion is scarred by the power of unreason, by the binding misery of fearful *tabus*, by the premeditated enforcement of human cruelty and suffering for the

KINGSHIP AND THE GODS

sake of preconceived notions about the way of ultimate salvation. Yet an awareness of other values shines like a beacon light through the darkness and emboldens us to hope :

Rejoice thou entire land, the goodly time has come
A lord is appointed in all countries. . . .
O all ye righteous, come and behold!
Truth has repressed falsehood.
The sinners are fallen on their faces
All that are covetous are turned back.

That was the song composed in Egypt at the accession of Merenptah. And of Hammurabi in Babylon it was said that he was called by Anu and Marduk 'to make justice appear in the land, to destroy the evil and the sinful, to prevent the strong from oppressing the weak'. Nor should we forget one New Year's day in the twenty-fourth century B.C.

That day when the king entered the temple . . .
And for seven days
The servant competed with her mistress
The domestic was on a par with his master.
In the town the mighty and the humble slept side by side ;
An evil tongue changed its words to good ones.
All evil was driven out.
He (Gudea) paid heed to the laws of Nanshe and Ningirsu,
Left not the orphan at the rich man's mercy
Left not the widow at the mercy of the strong.
The house which had no son—
Its daughter became its heir.
Days of righteousness dawned for him,
And on the neck of lawlessness and rebels he set his foot.

Glory to God in the highest, and on earth peace, good will toward men. The King is dead, Long live the King !

Men, Machines and History*

by O. G. S. CRAWFORD

DURING the present century there has appeared a new kind of history, concerned not with parts but with the whole of human history. There were, of course, before this, many attempts to write 'Universal History'; but they consisted merely in lumping together in a single continuous narrative condensed histories of the chief countries of the Old World. This synthetic or accretive method has the same defects when concerned with the sequence of time as it has when concerned with space. You cannot make a correct map of the world or of any part of it simply by sticking together on a single sheet the plans of separate portions surveyed independently by different people, with no comprehensive framework binding together the whole area. Nor, to vary the metaphor, is a cathedral constructed by putting several parish churches together beneath one roof. The new history is related to the Old Universal Histories exactly as is a modern map of the world to such medieval concoctions as the Hereford map.

The analogy, is, as always, imperfect. Most of the early cartographers realized the need of some rigid terrestrial framework and some, like Ptolemy, constructed one which was correct in principle and defective only from errors of observation and inference, or lack of data. The Arab geographers constructed an artificial system from Ptolemy and their own imagination. Modern maps are correct because they are based solely on the objective facts of observation controlled by mathematics.

To what extent is it possible to construct an objective framework for human history? Before attempting to answer this question we must be quite clear about what we are dealing with. The unit of cartography is the surface of the earth; of the new history, man as a species and societies of men. That we are able to stand back and see this vista at all is entirely due to the discoveries of prehistoric archaeology during the last hundred years. Those discoveries put man in his place in nature, replacing the speculations hitherto dominant by objective facts consisting chiefly of tools (such as stone axes) and skeletal remains.

Archaeology, by concerning itself exclusively with the remains of material culture, has thus created an objective framework for the history of man. It shows how a primitive anthropoid climbed up from the apes by not only using and making tools (which apes can do) but also by thinking about them and improving them (1). The range of

* (1) *Men, machines and history*, by S. Lilley. Cobbett Press (Past and Present Series: double volume). 240 pages. Price 10s 6d.

(2) Ikhnaton: the Great Man versus the Culture Process, by Leslie A. White [University of Michigan]. *Journal of the American Oriental Society*, Vol. 68, No. 2, April-June 1948, 91-114.

(3) On the use of tools by Primates, by Leslie A. White, *Journal of Comparative Psychology*, Vol. 34, No. 3, December, 1942, 369-74.

(4) *Joseph Stalin: an interview with the German author, Emil Ludwig*; printed by the Cooperative Publishing Society of Foreign Workers in the U.S.S.R., Moscow, 1932.

¹ For the development of this thesis see Leslie White, 'On the use of tools by primates', *Journal of Comparative Psychology*, Vol. 34, No. 3, December 1942, pp. 369-74; and his note, condensing his remarks, in *ANTIQUITY*, XXII, pp. 210-11.

MEN, MACHINES AND HISTORY

consciousness is thus gradually extended from a moment to millennia. Our modern consciousness of history is but an extension of our ancestral anthropoid's thoughts about his flint tools and how to improve them and pass on his knowledge. Language of course was a fundamental factor in this advance. Next we see man passing on from the hunter to the agriculturist and thus by rapid stages to urban civilization.

I wish to stress the objective character of the framework which archaeology has thus created. It is, of course, open to critics of a certain school to say that this framework is merely an inference from the facts. The criticism comes badly from those whose framework is often, on the most generous assumption, constructed of inferences unsupported by any objective facts, and it may be ignored.

From all this it follows that the new history is essentially of the kind called technologic. It is concerned with the fundamental needs of life—food, clothing and shelter, transport, raw materials, trade. It recognizes the class divisions which have bedevilled history for some five thousand years, and measures the level of civilization 'not only by its peaks of intellectual culture, but also by the standard of living of its whole people (2)'. Thus it redresses the balance of the other histories which have for the most part been concerned with the culture of a single dominant class. That is not unnatural. Down to modern times historians have been dependent on histories and annals which were themselves compiled by members of a ruling class which 'saw the world only from the point of view of consumers'. They do not therefore tell us the sort of things that the new historians want to know. There are pages and pages of politics and war, but not a word about how their daily bread was ground (3). It has been said that huge tracts of history are biased because 'God has written all the books'; it is equally true that all ancient history is equally biased because the books have been written by members of the ruling classes, interested 'in the art of exploitation, of government, of extracting the last available grain of corn from the peasant', but 'incapable of advancing the technical methods of society (4)', and therefore not interested in describing them—and indeed too ignorant of the processes to do so (5).

Modern discoveries and the publication of records has done much to redress the balance, particularly in economic matters. We catch a glimpse of the ordinary citizen at work or (more often) at loggerheads with another such. From the exhaustive study of all available records of a medieval town we can build up in our minds a fairly accurate picture of its social life. But here archaeology can often greatly help. What, for instance can we gather from the whole mass of documentary evidence between say A.D. 600 and 1066 that gives us the slightest idea of what a Saxon village or town was like? Practically nothing. Yet a couple of seasons' excavation on the site of Saxon Southampton tells us a great deal. It shows that this community was, so far as material culture is concerned, on or even below the level of similar British communities during the prehistoric Iron Age, a thousand years earlier. (Their huts were no better and their pottery definitely inferior).

The new history is concerned with the *whole* of any given society and not merely with the dominant class. It judges any such society by the cold criteria of their material

² Lilley, pp. 35-6.

³ The history of water-mills is an example. It had to be reconstructed from a few meagre references in the ancient writers who were no more interested in such matters than are their modern students.

⁴ Lilley, p. 18.

⁵ Malthus (1766-1834) said the same thing.

ANTIQUITY

culture, which includes of course works of art (if any). It is not impressed by religious labels, but only by behaviour. For instance, the unbiased historian sees much in common between Ireland and Abyssinia during the medieval period. Both societies were called Christian; both were geographically remote from civilized regions; both were literate but barbaric. Neither society made any effective contribution to technical progress. Both however are extremely interesting to the new historian because their fairly full documentation gives a vivid picture of the *sort of* life led by similar societies in undocumented periods.

It is not possible to form a true picture of these and other societies if one ignores their material culture, but only by comparing them with each other on that basis. The older historians and travellers, blinded by prejudice or obliged to conform to it for fear of consequences, regarded Christian societies everywhere as *ipso facto* superior to others. Muhammadans did the same. To each the societies of the other belief were infidel. Yet the discerning historian sees little if any moral or cultural differences that can be attributed to religion. The Christian Abyssinians castrated their enemies and gloated over it; and the records were set down by priests. The Muhammadan Turks impaled them alive down to the 19th century. The lives of the common people, made miserable by the misdeeds of their rulers, were much the same everywhere.

To sum up the argument so far:—The new history differs in kind from the older syncretic Universal Histories. It is an outcome of the scientific study of archaeology which provides an objective framework for the history of man, based upon the remains of his material culture. The picture thus composed is more complete than one drawn from documentary sources only, for these emanate from members of one section, the ruling class, and reflect its point of view. The new method lays bare the foundations upon which the superstructure rests and without which it cannot exist.

The argument now advances a stage. To continue the analogy one would say that (in the development of human society) the plan of the foundations determines the character of the superstructure. If the plan is rectangular, so are the upper stories. Translated into the language of history, this may be expressed by saying that technologic factors determine the social and political organization. That this is so may be seen most clearly in the early stages of human history. The primitive societies of nomads and hunters are based upon kinship ties. The wandering life prohibits complex political organization. But as soon as the basis of subsistence changes to agriculture, a new form of society immediately begins to develop. 'The transition from primitive to civil (6) society was brought about by technological advance, specifically, by the development of agriculture, supplemented—though not everywhere—by the domestication of animals. The maturation of the agricultural arts produced the following chain of sequences: increased food supply, increase in population, increase in population density and in size of political groupings, diversion of human labour from food-producing to specialized arts and crafts, a new type of exchange and distribution of goods, money and markets, economic classes, and so on'. The civil society which agriculture thus brought into existence 'required a special mechanism to coordinate the various segments and classes of society, and to integrate them into a coherent and effective whole. Such an integrated mechanism was produced. It was the "State Church", i.e., a mechanism having temporal and ecclesiastical aspects'.

⁶ 'The evolution of society is marked by two great stages: primitive or tribal, and civil or national. The tribe and clan are characteristics of primitive society (although the clan is by no means universal); the political state characterizes civil society'. White, *The Great Man*, p. 95, where the passage quoted above also occurs.

MEN, MACHINES AND HISTORY

We need not for the moment follow Professor White's particular argument any further. The first part (summarized above) will already be familiar to readers of Professor Gordon Childe's books, and probably therefore to most readers of *ANTIQUITY*; and the rest leads up to the discussion which is the special subject of the author's article, namely, the rôle of the Great Man in history. What we wish to make clear, even at the risk of wearisome repetition, is the fact, securely established by archaeology, that the great transition from primitive to civil society came about as a result of the development of agriculture. The (economic) foundations determined the (political) superstructure. If that happened once, it should happen again. Any comparable change in the mode of production should profoundly change also the political organization of society.

The change from primitive to civil society was the outcome of what Professor Childe (who first described it) called the First, or Neolithic, Revolution. Mr Lilley calls it the First Industrial Revolution; and his Book, which gives a condensed summary of it, is more particularly concerned with the Second Industrial Revolution. It is therefore the sequel to Professor Childe's studies—a sequel whose need was obvious, in order to carry the story down to modern times. It is therefore the last of a series of books which, taken as a whole, present a fairly complete outline of what I have here called the new history. The Second Industrial Revolution is divided into four phases: Embryo (A.D. 500–1440): Childhood (A.D. 1440–1660): Youth (A.D. 1660–1815): Maturity (A.D. 1815–1918). The factor which distinguishes the Second from the First is the application of mechanical, instead of human or animal, power to production, beginning with water and wind, and advancing to steam, electricity, oil (and now presumably nuclear fission). Naturally the book deals chiefly with mechanical inventions; but, unlike other such books, it is not merely a dry catalogue of dates and facts for the social implications and effects are constantly pointed out. The author's treatment of printing is an excellent example. Books, which had hitherto been valuable treasures because of the costly process of hand copying, [were brought] within the range of wider classes of people. Within a century or two the ambitious craftsman had the means to study in books the accumulated study of others and to apply it to his own problems. This was a revolution comparable to that brought about by the introduction of iron. Iron democratized physical tools; printing did the same for the tools of thought. This was an important factor in the increasing rate of invention that came in the following centuries (7). The claim is substantiated by many of the illustrations and references in the book.

It should be noted that Mr Lilley's treatment of history is essentially that of the archaeologist (8). He deals with the remains of material culture and from them builds a historical narrative. His is the sort of book that, deprived of documents, an archaeologist of the future might write, if the evidence were available to him. It is one of a series called 'Past and Present', whose editorial board consists of Professors Gordon Childe and Benjamin B. Farrington and Mr Sidney Herbert. Others in the series are Dr Grahame Clark's *From Savagery to Civilisation* (reviewed in *ANTIQUITY*, xxii, 1948, 217–8), Mr Cecil Curwen's, *Plough and Pasture*, and Professor Gordon Childe's *History*. It is aimed to publish forty books in all. If the others are all as good as these, the series will take its place in the history of publishing with 'Everyman's' and the Home University Library of our youth.

At this point we might well conclude this section. Some readers will certainly

⁷ Lilley, p. 47.

⁸ A minor point of criticism is concerned with his references to the wheeled plough, which need revision.

ANTIQUITY

think that it would have been better to do so. They are those who do not approve of the attempt to interpret contemporary or recent events in the light of the past. Admittedly there are dangers in mixing past, present and future, and there are occasions when it is desirable, or more politic, to remain aloof in the Ivory Tower. But this is hardly one of them. A review is nothing if not the expression of a personal opinion, and an opportunity for airing views, and so testing their validity.

Does it not follow almost automatically from the historical facts above enunciated that the Second Industrial Revolution should change the political organization of human society? If the First did so, surely it *must*? Like it, the Second has already enormously increased the population, and raised the standard of living. Will it not ultimately transform society as completely as did the invention of agriculture? What will life be like if it does? To these questions there are of course no answers except guesses. One thing is fairly certain and (to the present writer at any rate) reassuring—the process of transformation, though it may be much more rapid than the other earlier one, will be spread out over a considerable period, and will not take place everywhere at once. Even today, some seven thousand years afterwards, there still survive quite a few non-agricultural societies (Esquimaux, Bedouin, Beja, pygmies); and it seems probable that in remote places there may long survive a few non-collective farmers living in unrefabricated houses and eating untinned food. If amongst them there is room for uncontrolled archaeologists, it is probable that not only the present writer but many also of his readers will be found there.

These, however, are merely the irrelevant personal reactions of one whose life began and whose habits were formed in a world that is passing away. If we can judge by the reactions of the nomads of Arabia to-day, the paleolithic hunters probably regarded the way of life of the first farmers with similar aversion.

Human history thus falls naturally into three periods:—A very long period of hunting and collecting, a short period of some ten millennia of food-production based on agriculture and of industry based on human and animal power, and a period, which is just beginning, of both agriculture and industry based upon mechanical power. It is archaeology which has discovered the whole of the first period and the greater part of the second, and so created the new history.

The difference between the two kinds of history—the old and the new—is essentially one of scale—in time. Except during the two great revolutions mentioned there is not, in the short periods covered by national histories, time enough to show much technical advance. The scale is too large. Again there is a close analogy to be drawn with spatial scale represented by maps; a map of Switzerland covers too small an area on too large a scale to show the river system of Europe or the structural connexion of the Alps with the Pyrenees and the Carpathians. It is not of course intended to do so; but it does not therefore follow that a small scale map of Europe bringing out these connexions is not required, or that it is not a map at all. Yet there are some who would deny the name of history altogether to that small scale, long range kind whose unit is mankind, not parts of it. Of course both are needed; the small scale history of man is as necessary as the large scale histories of nations, just as there are times when a map of Switzerland is more useful than a map of Europe. But we shall probably get more out of our map of Switzerland if we have also studied a map of Europe; the Rhine, for instance, will have more meaning. The analogy is closer if for topographical maps are substituted geological ones, which reveal structure and suggest (to the trained eye) causation.

The analogy chosen has force because it is drawn from the other dimension, space; history is concerned with time. Both are quantitative concepts and susceptible of

MEN, MACHINES AND HISTORY

quantitative treatment. Scales are merely mathematical proportions, whether of time or space.

Professor White, as we have seen, accepts the view that technological advance is the mainspring of human history. His essay is designed to show that 'cultural advance' is 'not the work of a relatively few gifted individuals', and that the Great Men of the history books only seem to shape civilization. He traces the Great Man idea back to the magician who claims to be able to influence nature, and to his successors whose claims were greater, and were accepted. Some were regarded as better *shamans* than others; they had more 'power'. 'On higher cultural levels we find chiefs and priests; then kings and emperors, popes and potentates . . . As culture advances, the exceptional person increases in stature; great cultures [it is thought] can be built only by Great Men. Like Yahweh who made the light merely by calling for it, the Great Men make society and history by exercising their inherent genius'. This blind exaggeration of the power of the individual persists even today; and Professor White has selected for the purpose of demonstrating its falseness the example of Ikhnaton, 'the Heretic King'. He examines the claims made and shows—we think conclusively—that of Ikhnaton himself we know little or nothing, and that his character has been built up entirely by inference from the known events of his reign, often wrongly interpreted. The clinical diagnosis of his body is inconclusive, and it is even uncertain whether the body examined was that of Ikhnaton or not. 'The stirring events of [his] reign can be accounted for as a part of a great process of cultural change and development. And we can explain this process in terms of itself . . . in exactly the same way that we can account for the changes brought about in American culture by the introduction of the automobile. We do not need to call upon great men or upon psychological forces to make them intelligible'.

Ikhnaton, in fact, was merely the figurehead in an age-long struggle for power between State and Church—the two integrative and rival mechanisms of society. The State party was alarmed at the growing temporal power of the priests and decided to have a show-down. For a time they succeeded, but in the end the priests won. Their power was too firmly established upon this earth to be overthrown, and eventually the throne was usurped by the high priest of Amon who became successively viceroy of Nubia, commander-in-chief of the army, vizier of Upper Egypt and Pharaoh.

The anthropomorphic view interprets the known facts of history—of the culture-process (9), as Professor White calls it—by the pseudofacts of psychology, the known by the unknown. 'A worse error of reasoning would be hard to find—within or outside the field of scholarship'. What, he asks, 'could a man . . . of exceptional quality and ability have done in this or *any* situation except respond to it—to work with the materials at hand, to try to cope with the problems confronting him, in short, to fit himself to the culture process that is his context? A man of superior . . . make-up might have made a better, i.e., more effective, response than one of inferior brains and physique, but the pattern of his response would have been substantially the same because it would have been determined by the same cultural situation . . . So that even if Ikhnaton were an organism of exceptionally fine quality, this fact would not at all suffice to explain the events of his reign'. Actually, of course, as has been said, we know practically nothing about the quality of Ikhnaton, and what has been offered to us by the Egyptologists is either unjustified inference or pure fiction.

It is very interesting to compare this view of the rôle of the Great Man with that expressed by a Great Man himself. Opinions differ of course very widely about the

⁹ Prof. White uses the word 'culture-process' to refer to culture in its dynamic rather than its static aspect.

ANTIQUITY

character of Marshal Stalin, but very few, we think, would be so bold or so foolish as to deny him a place amongst the great men of history. On December 13th, 1931, the German journalist Emil Ludwig had an interview with Stalin and asked him several questions of a historical kind. The subject was introduced by Ludwig, who, after disclaiming any knowledge about economics but claiming to be a good judge of people, asked Stalin whether he thought there was any parallel between him and Peter the Great? Did Stalin regard himself as continuing the cause of Peter the Great? Stalin did not; historical parallels, he said, are always dangerous, and this question was absurd. He proceeded to show why. Lenin was mentioned, and this gave Ludwig the opportunity of suggesting that the historical importance of Lenin (admitted by Stalin) contradicted the Marxist view which (according to Ludwig) 'denied that personalities play an important rôle in history'. The reply must be quoted in full.

'Marxism', said Stalin, 'does not deny that prominent personalities play an important rôle, nor the fact that history is made by people. In *The Poverty of Philosophy* and in other works of Marx you will find it stated that it is people who make history. But of course people do not make history according to their fancy or the promptings of their imagination. Every new generation encounters definite conditions already existing, ready made when that generation was born. And if great people are worth anything at all, it is only to the extent that they correctly understand these conditions and know how to alter them. If they fail to understand these conditions and try to change them according to their own fancies, they will put themselves in a quixotic position. So you will see that, precisely according to Marx, people must *not* be contrasted to conditions. It is people who make history, but they make it only to the extent that they correctly understand the conditions they found ready-made, and to the extent that they know how to change those conditions. That at least is the way we Russian Bolsheviks understand Marx; and we have been studying Marx for a good many years'.

How intriguing is the contrast here brought out! Both Marshal Stalin and Professor White break up a given historical situation into the same component parts (so it seems to me)—on one hand the 'definite conditions already existing' (Stalin) and the 'cultural situation' (White) which the man on the other hand must understand and which are his context. But how different the opinions about the reaction of the man to the situation! White is a cultural determinist; the cultural situation determines the response of the man, and the pattern of this response will be substantially the same whether the man be superior or inferior. Stalin is the champion of the Great Man who can, if and in so far as he correctly assesses the situation, alter it. Perhaps the contrast is less profound than at first appears. Is not the existence of classes with opposed interests common ground to both? And is not the only difference, concerned with the extent to which an individual can influence the class struggle? After all it is not so much Ikhnoton (who was obviously a puppet) as the historians who have overrated his importance that Professor White is out after. He is not out to decry the Great Man so much as to confute those who do not understand correctly his historical rôle. Some (but not the present writer) will be startled to hear a voice from capitalist America decrying the historical rôle of the individual, and one from the U.S.S.R. proclaiming that 'men are at some times masters of their fate'. But did not Gordon Childe once write a book called 'Man Makes Himself'? Perhaps those startled ones, whose study of Marxism is probably derived from the letters written to *The Times* by infuriated and frustrated opponents, may still have something to learn about it!

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- THE MEARE LAKE VILLAGE: a full description of the excavations and the relics from the eastern half of the west village, 1910-1933, by ARTHUR BULLEID and HAROLD ST. GEORGE GRAY. Vol. I, 1948. 42s. (To be obtained from H. St. George Gray, Taunton Castle, Somerset).
- ROMAN WAYS IN THE WEALD: illustrated with maps, diagrams and photographs, by I. D. MARGARY. Phoenix House, London, 1948. 25s. [An account of field-work, excavation and air-photograph, extending over many years: foreword by the Editor of *ANTIQUITY*].
- ALEXANDER THE GREAT, by W. W. TARN. Cambridge University Press, 2 vols, 1948 (Vol. I, 10s 6d; Vol. II, 30s).
- PERSEPOLIS TREASURY-TABLETS, by GEORGE G. CAMERON. Univ. of Chicago Press: [Oriental Institute Publications, Vol. LXV]: London; Cambridge University Press. \$12.00.
- ONCE AGAIN ARTHUR'S BATTLES, by KENNETH JACKSON. *Modern Philology*, Vol. XLIII, No. 1, August 1945, 44-57.
- STRATIGRAPHIE COMPAREE ET CHRONOLOGIE DE L'ASIE OCCIDENTALE (3^e et 2^e millénaires), par CLAUDE F. A. SCHAEFFER. Published on behalf of the Griffith Institute, Ashmolean Museum, Oxford, by Geoffrey Cumberlege, Oxford University Press, London, 1948. 84s. [A masterly synthesis of all important excavated sites in this area, including his own at Ras Shamra].
- EXCAVATIONS AT THE JEWRY WALL SITE, LEICESTER, by KATHLEEN M. KENZON. Printed at the Oxford University Press for the Society of Antiquaries of London and the Corporation of the city of Leicester: sold by Quaritch, 11 Grafton St., W.1. 30s.
- ANCIENT INDIA, Number 4 (double number of 321 pages); to be obtained from the Director-General of Archaeology in India, New Delhi, India. Subscription rates for 4 numbers (2 years), 7 rupees or 11 shillings. [And remarkably good value! Articles by Gordon Childe (megaliths), Stuart Piggott (pins and a mace-head from Harappā), Ghosh (Taxila, with plan), Wheeler and Piggott (Iran and India in pre-Islamic Times) and by Wheeler on Mysore megaliths].
- SUMER, Vol. v, No. 1, January 1948. Published by the Directorate-General of Antiquities, Baghdad, Iraq.
- A HISTORY OF CAST IRON IN ARCHITECTURE, by JOHN GLOAG and DEREK BRIDGWATER. George Allen and Unwin, 1948. 63s. [A very fully illustrated account of the subject, covering much neglected Victorian iron-work, railings, etc.].
- HISTORY OF ANCIENT GEOGRAPHY, by J. OLIVER THOMSON. Cambridge University Press. 42s.
- GALLO-BRITISH COLONIES: THE AISLED ROUND-HOUSE CULTURE IN THE NORTH, by SIR LINDSAY SCOTT. *Proc. Prehistoric Society*, NS. Vol. XIV, 1948, 46-125. [A very important study of prehistoric house-plans by the President of the Prehistoric Society, who has excavated many of those described himself. This will become a *locus classicus*].

Reviews

TOPOGRAPHY OF ROMAN SCOTLAND, NORTH OF THE ANTONINE WALL. By O. G. S. CRAWFORD. 4to, pp. I-XII, I-162, figs. 1-32, pls. I-XXI, one folder and two maps on end-papers. Cambridge University Press, 1949. Price, 25 shillings.

This admirable and beautifully reproduced survey of the Roman roads, forts and marching-camps north of the Antonine Wall is the first comprehensive study of this rich and remarkable material since General Roy's great folio. Its author is one whose single-handed contributions to the antiquarian topography of Britain have placed him in a unique position among archaeologists and have set a standard of clarity and vigour peculiarly his own.

The study records the state of knowledge in 1943. Since then a good deal has been discovered in these very regions, more marching-camps at Ardoch, new marching-camps at Bridge of Allan and in the Mearns; and the fort-site at Bochart, near Callender, has been verified by excavation. But the value of Mr Crawford's work, as so often, is none the less still the challenge which it presents. The new discoveries at Bridge of Allan may seem to focus the view of the Forth crossing: in particular, they ask whether the Romans did choose the seriously overlooked site below the shadow of Stirling Castle, or a clearer site north of the river. In other words, was the bridge-head north or south of the Forth? Again, in clearing out of the way, most satisfactorily, existing identifications of Roman roads in the neighbourhood of Dealginross and Fendoch, the question is raised whether there were any at all and if so, where? The account of the signal-stations from Ardoch to Gask (why is there, however, no map of the Gask area?) raises the question whether the differences not merely in size but in plan are differences in age: just as the very interesting study of the Bertha region raises questions of the same kind. Roy, by the way, calls the marching-camp Grassy Walls, which seems more reasonable as a place-name than Grassy Wells, the author's version *passim*, and one would like to have seen the reason for the change discussed. Another site worth a text-figure is Cardean, of which the account is difficult to follow unless one knows the ground already. If Fochabers was worth a text-figure, so was it.

The historical discussion is weakened by the fact that it takes little account of other than Agricola's campaigns. If one may take liberties with Horace, *vixere fortes post Agamemnona*! And the relationship of the Ardoch marching-camp to the signal-station will suggest that we might make a start in definition by testing there. But I do not think that Raedykes has before been selected for the battle of Mons Graupius by a recognized authority: and there is much in the suggestion to commend it. I. A. RICHMOND.

THE ALPHABET: A Key to the History of Mankind. By DAVID DIRINGER, D.LITT. *Hutchinson's Scientific and Technical Publications*. London, 1948. pp. XII, 607. Figs. 256. 50s net.

This is not so much a book as an encyclopaedia. Here the last word has been said for many years to come on the Alphabet and the history of writing. It is sixty years since Isaac Taylor's book on the Alphabet appeared, and during the last half-century great advances have been made in this field. Every available scrap of new knowledge has been carefully gathered, sifted, and scientifically arranged by Dr Diringer in the

REVIEWS

volume of over 600 pages which now takes its rightful place as the definitive authority on everything concerning man's most important invention. Dr Diringer was already well-known to students of Semitic epigraphy for his Italian publications on North-Semitic inscriptions, and for his book on the Alphabet, also in Italian. Now he has produced, in English, an entirely new book on the Alphabet and on the history of writing in general. Great praise is due to the liberality and enterprise of the publishers, Messrs Hutchinson, for the lavish scale on which the book is illustrated, adding enormously to its value.

The book is divided into two parts. The first, occupying 191 pages, deals with what Dr Diringer calls 'Non-alphabetic Systems of Writing'; under this head are included the various forms of cuneiform writing, the Egyptian hieroglyphic, hieratic, and demotic scripts, the Cretan scripts, the Indus Valley script, Hittite, Chinese, Central American, and other ideographic scripts. A separate chapter is given to syllabic systems of writing, and the last chapter of this section deals with what Dr Diringer calls quasi-alphabetic scripts, including early Persian cuneiform, and Meroitic scripts. The order of treatment in this section is mainly chronological. Some slight, but unavoidable cross-classification is occasioned by the separation of syllabic scripts from the earlier 'mixed' systems which, although pictographic in origin, became largely syllabic in the course of their development.

The second section of the book, from p. 195 to the end, deals with the origin and growth of the alphabetic principle of writing, and discusses in turn all the known forms of alphabet which have been or are still employed in writing. The Indian branch of alphabetic forms is treated with a fulness not hitherto found in any book on the subject. The two chapters devoted to the immense variety of Indian alphabets contain a mine of information of the greatest interest, here brought together for the first time.

That part of the book which will probably be most closely examined is the chapter which deals with the still undecided question of the origin of the alphabet. The burning point of interest at the present moment is to be found in the discussion raging round the Byblos inscriptions, concerning which a wide difference of opinion exists between MM. Dunand and Dhorme. M. Dunand believes that the so-called 'pseudo-hieroglyphic' script, in which a group of inscriptions discovered by him at Byblos in 1929 and succeeding years, were written, is the 'incunable', or cradle, of the alphabet. He considers that these inscriptions belong to the period of the 12th or 13th centuries, and that they were contemporary with the existence of the early Phoenician alphabet. Hence Dunand is prepared to date the invention of the alphabet as far back as the 19th century B.C., five or six centuries earlier than any date so far proposed for that event. Professor Dhorme, however, who has deciphered and translated the Byblos inscriptions, though his results have so far not been published*, does not agree with Dunand on this very early dating of the invention of the alphabet. Dr Diringer sums up his own position on this question as follows: 'According to my opinion, we can date the origin of the North Semitic alphabet, or of its prototype, which we can call proto-Semitic alphabet, in the 2nd quarter of the 2nd millennium B.C. In other words, the great event occurred probably in the Hyksos period, which is now commonly dated 1730-1580 B.C. There is no doubt that the political situation of the Near East in that period favoured the creation of a "revolutionary" writing, a script which we can properly term "democratic" in distinction from the "theocratic" scripts of Egypt, Mesopotamia, or China. All the other more important attempts at alphabetic writing, the early Sinaitic script, the early Byblian and the early Canaanite scripts, can also be attributed to the Hyksos period'.

* Since this was written Professor Dhorme has published his results in *Syria*, xxv, 1-2.

ANTIQUITY

With regard to the place of origin, Dr Diringer rejects Sinai as the home of the first attempts at alphabetic writing, and prefers Palestine or Syria. Since the impetus to this great adventure was probably utilitarian, it seems more probable that the need felt for a more expeditious means of intercourse than the cumbrous hieroglyphic or cuneiform systems should have arisen in the busy trading centres of the Palestinian or Syrian seaboard. Since the publication of Dr Diringer's book, an important article in the *Bulletin of the American Schools of Oriental Research*, no. 110, by Professor Albright has appeared, dealing with the Sinaitic inscriptions and giving translations of them. Professor Albright's conclusions are, in his own words: 'I consider the Proto-Sinaitic script as normal alphabetic Canaanite from the early 15th century B.C.'. Professor Albright has also carried considerably further the identification of the Sinaitic signs with corresponding signs in the early North-Semitic alphabets.

One valuable feature of this book is the full bibliographies which are appended to each chapter. Very little has escaped Dr Diringer's notice. In a book of this size it was inevitable that a few errors and mistakes should have remained uncorrected, but they are surprisingly few. 'Meisler' appears for 'Maisler', and on p. 564 the source of information concerning the strange Obei Okaime script is erroneously given as the *African Journal*, instead of *Africa*, the well-known quarterly of the International Institute of African Languages and Cultures. The uninitiated reader, and many such will be using this book, might find the meaning of the word 'syllabary' a little obscure, e.g., on p. 36 and elsewhere. Also the term 'stimulus-diffusion' might profitably have been further elucidated. The Index contains no references to authors, but their inclusion might have swollen it to undue proportions. But these are trifles in comparison with the magnitude of the achievement which this book represents. It will long remain the standard authority on everything relating to the history of writing. S. H. HOOKE.

ESSAI SUR LES ORIGINES DE LA MINIATURE DITE IRLANDAISE.

By F. MASAI. [des publications de Scriptorium, vol. 1]. pp. 146, plates. Bruxelles: Editions 'Erasmé', 226 Avenue des Alliés; 1947. Price 450 francs.

This study of the origin and sources of early Insular miniatures is best summarized in the author's own conclusion that henceforth it will be more precise to speak, not of Irish, but of Anglo-Irish miniature painting. 'Although the Scotti', he continues, 'practised this art during several centuries, thereby establishing a certain right to retain the older designation, the English, who were creators of the style and produced its greatest masterpieces, have no less vindicated the claim to give it their name' (p. 136). This conclusion is less novel than a superficial reading of M. Masai's work would suggest, but his thesis, stated polemically and developed with the aid of a learned array of authorities, is a useful summary of the trend of modern opinion, setting out a case which can no longer be ignored or relegated to footnotes. It must, however, be noted that the citations are eclectic and that the author does not hesitate to suppress evidence unfavourable to his cause. One instance must suffice. Dismissing the traditional ascriptions of the Book of Dimma and the Book of Moling, Masai (p. 41) writes: 'The name of Dimma is too common and the whole ms. speaks against a great antiquity. Zimmermann boldly attributes it to the second half of the 8th century. An interesting observation of Lowe should also be noted: the whole superscription on fol. 74^v is, according to the eminent palaeographer, a late addition. Furthermore in three other places (Lowe is again cited as the authority) the name of Dimma is written over an erasure . . . The other codex . . . bears at the end of St John's Gospel the following colophon *nomen autem*

REVIEWS

scriptoris mulling dicitur. This Mulling has been identified with the Bishop of Ferns, who died in 696. The decoration does not support this judgment and, as Zimmermann observes, the text has every appearance of a copy and must be of this second half of the 8th century at the earliest. The ignoring of Lowe's careful judgment (*Codices Latini antiquiores*, II, no. 276), that 'the identification with St. Moling (†696) the founder of the monastery, seems palaeographically possible', is difficult to explain, even though the author may have good reasons to dissent from it.

The Book of Durrow, the oldest and one of the finest of the illustrated Insular MSS. is fully discussed and the case for its Northumbrian origin is fairly stated. E. A. Lowe (*Codices Latini antiquiores* II, p. xiv) drew attention to the orderliness of the script, a Saxon, not an Irish, feature and its palaeographical connexion with the Codex Epternacensis. F. C. Burkitt (*Journal of Theological Studies*, xxxiii, 254; *ANTIQUITY*, ix, 33) pointed out the pure Northumbrian character of the text of the Gospels, which is closely related to that of the Book of Lindisfarne and of the Codex Amiatinus. Clapham (*ANTIQUITY* VIII, 43) shewed that the sources of the ornament were Saxon and that the greater number of the motives were unknown to Irish art of the 7th century. Zimmermann (*Vor-Karolingische Miniaturen*, 124) though accepting the traditional ascription to Ireland had already noted that the Durrow style gave rise to no imitations in Ireland but found its copyists in England and the English inspired monasteries on the Continent. The last two writers, the earliest in date of publication, accepted the traditional ascription of the MS. to Ireland but this rests in the last resort on no firmer basis than the famous colophon, with its reference to Columba the writer, and Abbott (*On the Colophon of the Book of Durrow*, p. 199) had long ago pointed out that the vital words were written over an erasure. The growing weight of evidence led Lowe to the clear attribution: 'written in Northumbria by a hand trained in the Irish manner' (*Codices Latini antiquiores*, II, no. 273) and this conclusion has not been seriously impugned, though more recently T. D. Kendrick, in a book apparently not known to Masai (*Anglo-Saxon Art*, published in 1938), has cited the palaeographical evidence and, after considering Clapham's suggestion that the decoration was Northumbrian, rejected this view and restated the 'Irish' case. The archaeological material is discussed by Masai in a separate chapter and here again the evidence points in the same direction.

In the later period interest will centre on the author's treatment of the Book of Kells. His analysis is less full and his conclusion less definite (p. 124): 'all the lines of evidence converge, all point to a Northumbrian workshop, or at least to a scriptorium dependent on (dans la mouvance de) Jarrow and Lindisfarne; such as in the wider sense (à la rigueur) the abbey of Iona, might be considered'. It is difficult to agree. Leaving aside the ornament on which opinions are likely to remain more subjective, one may turn to the script and cite the opinion of Lowe (*Codices Latini antiquiores*, II, p. xii): 'The Irish scribe often behaves as if the written line were something elastic, not a fixed and determined space, which has to be filled in a particular way. He seems often guided by whim and fancy. The English scribe by comparison, is balanced and disciplined. One need only place side by side the Book of Kells and the Lindisfarne Gospels, or the Book of Mulling and the Moore Bede to see the force of these observations'. In the reviewer's opinion the arguments adduced by Masai cannot outweigh this judgment. English influence is clear both in this text and the ornament. But English customs were reaching Iona as early as the time of Adamnan. The Book of Kells may well have been written at Iona and have absorbed the various influences at work in that centre; but if this were so, it seems idle to quibble at the use of the adjective Irish to describe a product of the monastery, which was still the seat of the successor of St. Columba.

ANTIQUITY

It is impossible to consider in detail the many issues raised by this book. One omission in the bibliography has already been cited. Another is Clapham's *English Romanesque Architecture before the Conquest* which contains a more modern and authoritative treatment of the great Saxon crosses than the work of Collingwood cited on p. 64. A number of errors have crept in. Ricemarch, the son of Sulgen, was a Welsh not an Irish scribe (p. 48); like his father he was Bishop at St David's and his death is recorded under the year 1099. Alfred, who redeemed the Codex Aureus and gave it to Canterbury, was not the King but an *ealdorman* as the translation on p. 62 shews. Attention may also be drawn to the author's theory of Insular majuscule and minuscule scripts, attributing the one to England and the other to Ireland (p. 139). A full consideration of this far-reaching suggestion would require an analysis of all the MSS., not only of those with illustrations, and would, in particular, necessitate an explanation of the position of the Cathach of St. Columba, which is generally classed as majuscule and which Lowe (*Codices Latini antiquiores*, II, no. 266) is prepared to accept as being of the time of the Saint. It is, at any rate, far earlier than Christian Saxon influence in Ireland. (Cf. Proceedings of the Royal Irish Academy, XXXIII, 291). But a discussion of these points would reach far beyond the limits of this review, in which it is unnecessary to consider in detail the consequences of Masai's main conclusion, consequences which he only claims to have sketched in outline.

C.A.R.R.

HISTORY IN LEICESTER. By COLIN D. B. ELLIS. *City of Leicester Publicity Department*, 1948. 138 pages, 25 maps and illustrations. Price 9s 6d.

Leicester and Leicestershire have been fortunate in their historians. They have had a Throsby, a Nichols and a Miss Bateson, while more recently C. J. Billson, R. W. Greaves, W. G. Hoskins, A. T. Patterson, S. H. Skillington and others have all done work of which any area might be proud.

Mr Ellis's book is something different. It is not a work of original research; it is not even a learned work (the earlier sections betray inadequate and insecure foundations), and the balance of interests, local, national and dramatic, is not always well maintained. But to criticize it as a work of scholarship or even as a history of Leicester is to misunderstand the author's aim. One feels that he loves his Leicester and is determined to make others love it too. He attempts the difficult task of weaving local and national history into a popular story that can be read and enjoyed by all. And, assisted by an easy racy style, he has succeeded. His sacrifices of completeness, of balance, and sometimes of accuracy, do not prevent the achievement of his primary aim which is, it seems, to attract. He has written an extremely attractive book and, as was no doubt his intention, it will drive many to deeper study.

Outstanding features of the book are its superb illustrations and its uniformly excellent production. And the title page bears the impress of the City of Leicester Publicity Department. Leicester is fortunate: it possesses a University department of Local History and an energetic group of local historians who can look with confidence for municipal support. This happy combination promises a bright future to local historical and archaeological studies in Leicestershire.

F. T. WAINWRIGHT.

Antiquity

VOL. XXIII No. 91

SEPTEMBER 1949

Editorial Notes

A DISCOVERY has been made in America which may be of the greatest use to archaeologists. It consists of a method of dating dead pieces of formerly living substances (such as wood and bone) by means of their radiocarbon content. The details are highly technical and beyond the comprehension of those who are not specialists. Preliminary tests on dated specimens of wood from Egypt have produced results which, as the table shows, agree very closely with the values expected; the maximum range of error is about 450 years, but the average of four samples agrees with the expected result to within about a hundred years. Further refinement of the process is expected to enable specimens as old as 20,000 or 25,000 years to be dated. That covers the whole of the Mesolithic period and takes us back to the end of the palaeolithic period and into the final phase of the Ice Age. Wood specimens are available (from peat deposits) for the whole of this period; and if the radiocarbon method, as it may be called, can be used on such, a very considerable refinement of the present British chronology will become possible. For the present methods of dating peat deposits rest ultimately on the absolute chronology built up on the basis of clay varies in Sweden, and on correlations between British and continental deposits. Other promising subjects are wooden piles from lake-dwellings, wooden handles of axes, prehistoric boats and coffins and all other organic material, such as the straw included and preserved in clay bricks and baskets, and seeds and fruit stones. We print below an extract from an article by Professors W. F. Libby, E. C. Anderson and J. R. Arnold, of the Institute for Nuclear Studies, University of Chicago, published in *SCIENCE*, March 4, 1949; Vol. 109, No. 2827, pp. 227-8; and we wish to thank the publishers for permission to do so, and Professor Libby and his colleagues for the same and for an offprint of the article. We also wish to thank Lord Cherwell for calling our attention to the discovery, which was first made known in this country by *SCIENCE TO-DAY*, Vol. 5, No. 125, 24 March 1949. We hope that it may be possible to publish in *ANTIQUITY* a full and authoritative account of the method, when further experiments now in progress have been concluded.

Having established the world-wide uniformity of the radiocarbon assay at the present time, it seems a logical assumption that this would have been true in ancient times. Assuming this, and using the half-life of radiocarbon, $5,720 \pm 47$ years, one can calculate the specific activity to be expected after any given time interval elapsed since the removal of any carbonaceous material from equilibrium with the life cycle. For living materials this probably coincides with the time of death; for carbonates it would correspond to the time of crystallization (assuming no further interchange with the solution or atmospheric carbon dioxide to occur). On this basis we have undertaken examination of wood samples of well established age from the ancient Egyptian tombs. Two such samples were used, one from the tomb of Sneferu at Mejdum (furnished by Froelich Rainey, of the University of Pennsylvania Museum, Philadelphia) which was $4,575 \pm 75$ years old; the other from the tomb of Zoser at Sakkara (furnished by Ambrose Lansing, of the Metropolitan Museum of New York) which was $4,650 \pm 75$.

ANTIQUITY

years old. The former sample is cypress wood; the latter is acacia. John Wilson, of the Oriental Institute of the University of Chicago, has given the dates quoted, at the behest of a committee of the American Anthropological Association, consisting of Frederick Johnson, chairman, Froelich Rainey, and Donald Collier. The expected assay for 4,600-year material is easily calculated to be 7.15 ± 0.15 cpm/gm of carbon on the basis of the present assay and the half-life. Table 2 presents the data obtained on these materials.

The data on both samples were averaged since the error in ages almost overlaps the difference, and the weighting was taken according to the error quoted in each run. The errors quoted here and in Table 1 also are standard deviations determined strictly from the statistical counting error, and, since the data agree within these errors, we believe that no other appreciable error is involved in the measurement. It is gratifying that the mean of the determination agrees with the expected value within 1 standard deviation unit. An error of 0.4 cpm/gm in the specific activity corresponds to an error of 450 years in a 4,600-year-old sample.

On this basis we feel encouraged to proceed with further tests on younger samples of known age. This work is now in progress. It is hoped that certain unknowns can be measured in the near future. A large thermal diffusion column similar to the one used by Dr Grosse and his associates has been installed in the laboratory and a considerable increase in accuracy should result, permitting the measurement of samples as old as 20,000 to 25,000 years.

AGE DETERMINATION ON THE EGYPTIAN SAMPLES

<i>Sample</i>	<i>Specific activity found</i> (cpm/gm of carbon)
Zoser	7.88 ± 0.74
"	7.36 ± 0.53
Sneferu	6.95 ± 0.40
"	7.42 ± 0.38
"	6.26 ± 0.41
Weighted average (both samples)	7.04 ± 0.20
Expected value	7.15 ± 0.15

With the present number of ANTIQUITY we are glad to record the accession of quite a large number of new readers, to whom we give a hearty welcome. We hope they will be satisfied with its contents. We try to act as a medium between specialists and the general reader; but we also like to be the first to publish new discoveries. Sometimes such articles are necessarily somewhat technical, but we know (from letters received) that readers appreciate them. That is especially true where an advance in technique is described, such as that recorded in the preceding paragraphs. One can appreciate its significance without understanding the methods. Dating sites and objects is the prime objective of archaeology, and absolute dating the ultimate objective. For until an orderly sequence of events has been established there can be no history, and without an absolute chronology the relations between cultures and civilizations must remain obscure and uncertain. A period that is not dated absolutely in years is like a map of an area that has no latitude or longitude; one cannot place it on the Earth's surface. If one could imagine such uncoordinated maps as existing separately of England and France, there would be nothing to show the existence of the English Channel, which has played so great a part in the history of both countries.

Ninety Years Ago

by JOAN EVANS

NO one, so far as I know, has as yet made any particular study of the many local learned societies that were founded up and down France in the years after the French Revolution, and of their effect upon contemporary thought. These Sociétés d'Émulation differed from the local archaeological and historical societies of England in being more all-embracing; they included in their scope pure literature, philosophy and science as well as the history and antiquities of the district, and often developed a philanthropic side as well. At the time of their foundation they were often of a free-thinking colour; but as the balance of French life came to be restored after the Revolution this element was forgotten, and the more learned priests of the neighbourhood were often included among their members.

These societies fostered a peculiar polymathic quality among those who regularly attended their frequent meetings. They were to a great extent self-supporting in the provision of papers and communications, and it would have been pure selfishness for any member with any claim to versatility to specialize too deeply. Their standards, too, were not those of the Metropolis, where a new professionalism was bringing higher and more exact criteria into every branch of knowledge and speculation. Rather, we can see in the Sociétés d'Émulation of the nineteenth century the continuance of the amateur spirit that in the eighteenth century had flourished in the aristocratic circles of Court and château: a spirit surviving in a less polished form among the lesser gentry of the provincial towns.

The Société d'Émulation de la Somme, with Abbeville as its centre, was a typical society of the kind. It had been founded in 1795 by a Monsieur de Mautort and a Monsieur Boucher de Crèvecœur who rapidly rose from Secretary to President. His son, Jacques Boucher de Crèvecœur de Perthes, began to take a leading part in its councils after 1825, and in 1827 added a small museum to its activities and revised the statutes. In 1830 he in his turn became President, and in 1832 instituted the printing of its statutes.

Jacques Boucher de Crèvecœur de Perthes may be regarded as the fine flower of members of a Société d'Émulation. He was born in 1788, at Rethel, a scion of a good country family of the lesser nobility; his father, Jacques Boucher de Crèvecœur, had for reasons of prudence dropped all claims to nobility during the Revolution, but that made no difference to his pedigree. On his maternal grandfather's side Jacques came of yet more illustrious stock, for the said grandfather was the last direct descendant in the male line of Jeanne d'Arc's first cousin, who married Colas de Perthes. When this grandfather died in 1818 Jacques Boucher de Crèvecœur assumed the name of de Perthes by royal licence in order that the name of Jeanne d'Arc's closest kin should not die out.

His family contrived to live quietly through the Revolution, and the Terror without emigrating. His father had had some acquaintance with Napoleon in his days of obscurity and at one time entertained hopes of his son's becoming one of the Emperor's pages. Instead, in 1803, Jacques Boucher de Perthes entered the Customs and Excise, and in that service travelled a good deal in a rather picaresque fashion all round the

ANTIQUITY

Mediterranean. He returned to the Pas de Calais for the proposed invasion of England; held a post in Brittany for a time, and then in 1825 contrived to be nominated to a customs post at Abbeville, where his father had long been settled and where he had passed much of his boyhood. The post was unimportant; he had private means; and he was free to make the Société d'Émulation one of the chief interests of his life.

He had all the qualifications such a Society required: immense facility, and interests as varied as his ambitions. He longed to succeed as a dramatist; he wrote a vast number of less than mediocre plays, and never failed to be disappointed that he could not find anyone to produce even one of them in Paris. It remained one of the great disappointments of his life that though in 1823 he succeeded in getting his tragedy of *Persée* accepted for the Odéon, it was never performed. He had a capacity—not uncommon in men brought up on the windy oratory of the Revolution—of composing orations on general subjects: courage, the place of women in society, and so on; he rhymed easily, and had a satirical turn; he wrote “characters” in the manner of La Bruyère, and he compiled a work in five volumes *De la Création*. All these works formed the basis of discourses or readings to the Société d'Émulation; and in addition he did all the things proper to a local society, from exhibiting a giant pear to founding prizes in the local orphanage.

Such work would not have brought him fame; but besides general culture the society was interested in local antiquities, as was natural in a body that had its home in so long-inhabited an area as the valley of the Somme. M. Casimir Picard, a local doctor with a great deal less facility and a great deal more exactitude than Boucher de Perthes, had been interested in flint axes as long ago as 1830 (1). In 1836 (2) he made a communication to the Society on stone axes of a ruder type than the regularly shaped examples that were familiar to those who collected the objects found in the peat cuttings of the Somme valley. He claimed the ‘haches dites ébauchées’—what we should now call palaeolithic flint implements—as being an original and early type of axe, not unfinished examples of the other more familiar kind. He had, moreover, recognized flakes as an early form of tool, and had established the relation of such flakes to a core. Picard had inherited some notion of stratification from Traulle’s and Mongez’ work, and on 20 November 1835, set out its importance before the Société d'Émulation and planned excavations to establish the local sequence. Unfortunately he was a busy professional man and was slow to undertake further researches; and, more unfortunately, in 1841 he died rather suddenly.

The mantle of Dr Casimir Picard fell upon the shoulders of Jacques Boucher de Perthes, who wore it with such dramatic effect that he soon forgot that it had not always been his own. He needed something fresh to divert his mind, for he had been seriously disappointed in several projects. He had planned to secure a nomination to an excise post in Paris, which was to fall in about 1843, in three years time; and to secure for himself more consideration in the city than he might otherwise have enjoyed, planned to offer his collections of antique Picardy furniture and miscellaneous objects to form a separate gallery in the Musée de Cluny which was being founded by the generosity of M. du Sommerard. By 1843 he wrote in very handsome terms to Viollet le Duc to offer his collections to the State, on condition that he was to have the arrangement of them (3). Viollet le Duc was slow to answer, but Boucher de Perthes was quick to write to every

¹ Aufrère, *Épreuves*, p. 6.

² Mem. Soc. d'Ém. d'Abbeville, 1836–7, I, 234.

³ *Sous dix Rois*, v, 400. Many of these letters are suspect, but I think this is authentic.

NINETY YEARS AGO

possible interested administrator in turn, to offer his fifteen to twenty oak cupboards and as many chests, and his medieval carvings, in return for a gallery bearing his name and a marble plaque recording it (4). In August 1843 the offer was accepted, and he was almost as much pleased as when the Odéon had accepted *Persée*. Once more, however, an endless succession of unaccountable delays prevented the scheme from reaching fruition. The authorities of the Musée de Cluny made a great fuss over the weight of his oak cupboards and the difficulty of transporting them from Abbeville to Paris. Finally, in October 1848, the gift was refused on the ground that it was not of sufficient artistic importance to be specially housed; and it did not enter into Boucher de Perthes' plans to have his collection merged in those of M. du Sommerard, even though he was offered a secondary position on the Museum staff.

In the meantime he had realized that his Parisian ambitions were unlikely to materialize, and had sought fresh laurels in the geological field. His amateur status in that field was unquestionable. In the years before 1825 when he was stationed in Brittany he had accompanied his friend Général de la Truglaye on a few geological walks; and to watch him wield a hammer and endure the consequent fusillade of granite chips had, he felt, initiated him into the science. His previous book (in five volumes) on the Creation had not taken much account of so mundane a science, but he staked his claim to Picard's subject in his *Antiquités celtiques et antédiluviennes*, of which the first volume appeared in 1847.

He begins (5) by claiming to have visited practically every excavation of an engineering kind in the neighbourhood for the last ten years; the claim may be well-founded, for he was an idle man and a good walker. He divides early history, going backwards, into five periods: Gallo-Roman, Gallo-Celtic, Celtic, Pre-Celtic, and Diluvian, and defines the scope of his book as covering the last three. He gives, without acknowledgment to Picard or any other authority, a good account of the stratification of the Somme Valley in undisturbed ground: an average of 30-40 cm. of medieval remains, then some 50 cm. of Roman, and about the same of Gallo-Roman; then some 2 metres of Gaulish; then a metre down—or about 4 metres below ground level—one is in the middle of a 'Celtic' (or as we should now say, neolithic) stratum; and below this one finds an alluvial layer of varying depth (6).

He recognizes that below the strata that contain recognizable objects of human handiwork others are reached containing bones which show that the animals they come from have been used for food, presumably by men (7). Once more following in Picard's footsteps, he distinguishes between the polished well-shaped implements of the later period (which he calls Celtic or sometimes pre-Celtic and identifies with the age of dolmens) and the ruder ones, the *haches ébauchées*, which he calls diluvian, or sometimes antediluvian. He rightly notes the absence of pottery in the stratum that produces the *haches ébauchées*, and rightly stresses that though this stratum must cover an immense length of time the axes in it show no visible development on the sites known to him. Yet he falsifies the very foundation of his argument and the whole point of the discovery by accepting the neolithic axes which his workmen had introduced into the 'antediluvian' stratum in the sandpit of Menchecourt as being indeed of the earlier period (8), and

⁴ Aufrère, *Préhistoire*, p. 74.

⁵ p. 2.

⁶ p. 165.

⁷ p. 60.

⁸ pp. 27, 183.

ANTIQUITY

contemporary with the fossil remains of elephants and rhinoceros in the same stratum. (It is the greater pity because his recognition of the riverside dwelling, and the neolithic stratum on the same site are reasonable and valid). He is moreover, remarkably vague in his dating; he never commits himself to identifying the Flood that caused the stratum on which the dating of the *haches ébauchées* depends, but one suspects that he thought of it in strictly biblical terms.

His horrid little outline plates include a few scrapers and a number of flint implements of Acheulian type (9), some of which are included in his collection of 'early sculptures': flints in which he found resemblances to the human face, and to animal and plant forms. He lingers happily over his late neolithic 'Celtic' finds in bone and stone, and it is not until Chapter xvii (10) that he reaches his 'haches diluviennes', described in careful detail with considerable skill, even though no technical vocabulary for the purpose had yet been invented. (Whatever Boucher de Perthes lacked in the way of scientific acumen, he had the archaeological eye). He returns once more to the later period with his 'couteaux celtiques' and then comes to his 'couteaux diluviens' which for the most part are recognizable palaeoliths. But he shies away once more from the early stratum to continue with a chapter on dolmens full of fanciful analogies between their shapes and those of the worked stones; and then really enjoys himself in a dissertation on his imagined sculptures in flint.

It is not altogether surprising that few people took the book very seriously. His 'Celtic' (neolithic) antiquities were not unfamiliar and were acceptable enough; but his antediluvian objects were suspect from the start and had most disturbing implications of which Boucher de Perthes himself seemed to remain completely unaware.

He was once more trying to get to Paris, this time as a deputy. He stood for election to the National Assembly in the election held after the proclamation of the Republic, but failed to get in. Once more he turned to his collections. Once more he started on the familiar routine of writing to all the Parisian personalities. In September 1848 (11) Jomard and Constant Prévost came to look at the drifts where flint implements were reported to have been found and declared that they were tertiary, without giving any opinion on the implements themselves. That opinion he waited for and wrote about in vain. Archiac, in his *Histoire des progrès de la Géologie* (12), which had appeared in 1848, had discounted Boucher de Perthes' discoveries, and had damped the mild interest of the Academy of Sciences. In February 1849 Boucher wrote to M. Cordier of the Academy to ask for their final report, baiting his letter with an offer of the bone, horn and stone collections, including the antediluvian implements and sculptures, to their Museum. On 6 March, he received a report declaring that the stones were worked by man; but it was accompanied by a letter in which the authorities of the Academy declined the collections and declared their unwillingness to take any further steps in the matter or to send anyone else to inspect the objects or the sites where they had been found.

Paris was evidently closed to him, and his thoughts turned to London. On 24 March 1849 (13) he sent his book and some specimen flints to C. Roach Smith, Secretary of the British Archaeological Association, for exhibition at one of their meetings. They

⁹ See particularly plates xviii, xx, xxi, xxii and lxxvi, et seq.

¹⁰ p. 347.

¹¹ *Sous dix Rois*, vi, 197.

¹² ii, 166.

¹³ *Sous dix Rois*, vi, 215.

NINETY YEARS AGO

were duly shown to the Association on 25 April, but were not found worthy of record in the *Journal* though they are reported—probably by Thomas Wright—in the *Literary Gazette* of 28 April. The claim that they date from before the Flood is recorded, but no conclusion is drawn. Boucher de Perthes had to console himself with the qualified acceptance of his discoveries by C. Buteux in the second edition of his *Esquisse géologique du département de la Somme*; but this seemed to have only a local circulation. Dr G. A. Mantell mentioned his theories at the Oxford meeting of the Archaeological Institute in June 1851, but was more definite in refusing to accept his sculptures than in accepting his implements (14).

A pause ensued, in which Boucher de Perthes occupied himself with local activities, considerably increased in volume under the new Constitution. Only in September 1851 did he get to London (which he did not like) and see Roach Smith; but Roach Smith had collections of his own to show, and Boucher de Perthes got little satisfaction from the visit.

His great difficulty was to get any experienced observer to come and look at his pits; there was the sandpit at Menchecourt (with dishonest workmen who 'planted' flints in the places where they knew he wished to find them) and gravel pits at Saint-Acheul and Saint-Roch-lès-Amiens in which he took a lesser interest. Boucher de Perthes (who may have suspected that his own exposition of the finds was not very convincing) rightly felt that to get experts to look at his sites was the best way to convert them to his own view of their importance. In 1854 he had his first real convert. A Dr Rigollot, a good amateur geologist who had been interested in the fossil remains from Saint-Roch-lès-Amiens as long before as 1819, had always refused to take Boucher de Perthes' theories seriously. Now he made a fresh examination of the pits, was converted, and avowed it. He published a paper on the flints from Saint-Acheul (15)—a scholarly representation of Boucher de Perthes' material—and was nominated in 1855 a Correspondent de l'Académie des Inscriptions on the strength of it. He died before he could be admitted, and Boucher de Perthes tried in vain to get nominated in his stead. Once more fresh adversaries arose; the pious became anxious about the orthodoxy of antediluvian man in the valley of the Somme, and succeeded in fogging the issue; and geologists who had not visited the pits declared that the drifts were not untouched but man-made and of recent creation.

Rigollot's conversion and success spurred Boucher de Perthes to new efforts; once more he hoped to inherit the mantle of a dead prophet. In 1857 he published the second volume of his *Antiquités celtiques et antédiluviennes*: a volume rather thin in fresh content if not in bulk. He was, after all, nearing seventy. The significant passages are Chapter VI, in which he considers the relation between the 'celtic' (neolithic) and 'antediluvian' (palaeolithic) finds; he found nothing identical between them but the presumed flint sculptures of animals. In Chapter XII he discusses the fresh finds, especially at Saint-Acheul and Saint-Roch; and in Chapter XIX describes the main 'antediluvian' types—the triangular axe, which he considers the oldest, the knife with a flat butt and a lance-like point, which he considers typical of the middle stratum; and the triangular arrow, typical of the most recent. He also discusses the almond-shaped axe and the flake and scraper; the heart-shaped stone, and the disc, which he inclines to attribute to the Celtic period; the axe-hammer, the castanet-scraper (again Celtic) and other forms—which appear to be cores—for which he can find none but a symbolic use. The intervals are filled with general discussions, chiefly of the flint sculptures

¹⁴ *ArcH. Journ.*, VII, 1851, p. 210.

¹⁵ *Mémoire sur les instruments en silex trouvés à Saint-Acheul*. Amiens, 1855.

ANTIQUITY

which he was beginning to see everywhere. He had little chronological sense and happily includes Gaulish and Gallo-Roman sculptures in his plates ; but he sometimes had an intuition of truth, as when (in a note (16)) he asserts his belief in the gradual transformation of species under the influence of needs and environment.

Already, we may guess, he was conscious that people said he has derived his ideas of early man from Picard and Rigollot. He goes out of his way to declare that the notion that worked flints might be found in tertiary beds had first come to him in 1826 when examining a sandpit outside Abbeville (17), an assertion that can neither be proved nor disproved but cannot be unreservedly accepted.

The second volume made even less impression on the learned public than had the first. Littré, however, cited Boucher de Perthes and Rigollot in a literary article on Cave Men in the *Revue des Deux Mondes*, and on the whole came down on their side. In June 1858 the paper Boucher de Perthes read to an archaeological congress at Laon was ill received, in spite of Rigollot's publication. In August he wrote to Adrien de Longpérier, the Keeper of Antiquities at the Louvre, to suggest that a Musée Gaulois should be founded to contain the collections which he was desirous of presenting to the State (18). They are classified as antediluvian, antehistoric (a new name for 'Celtic') Gaulish, and Merovingian. There is enough, he declares, to fill 'une vaste galerie'. The offer seems to have been politely declined : his reputation was not standing at a high level in Paris, though when in November he proposed that the Société d'Émulation should commission a local painter to create a large and partly allegorical picture of all the celebrities of Abbeville and the neighbourhood (19) he doubtless expected to find himself in the front row.

Fate, however, that had kept him waiting so long for any expert examination of his case, now relented. At the beginning of November 1858 the English geologist, Hugh Falconer, who had already pointed out the significance of worked flints and bones of cave-bear and rhinoceros being found together in Brixham Cave, happened to pass through Abbeville and saw Boucher's collections (though not his sites) and at once wrote to his friend Joseph Prestwich (20).

'He showed me "flint" hatchets which *he had dug up with his own hands* mixed *indiscriminately* with the molars of *E. primigenius* . . . Abbeville is an out of the way place, very little visited, and the French savants who meet him in Paris laugh at M. de Perthes and his researches. But after devoting the greater part of a day to his vast collection, I am perfectly satisfied that there is a great deal of fair presumptive evidence in favour of many of his speculations regarding the remote antiquity of these industrial objects and their association with animals now extinct . . . If, during next summer, you should happen to be paying a visit to France, let me strongly recommend you to come to Abbeville'.

Prestwich had read of worked discs from Abbeville, not in Boucher de Perthes' own book but in Buteux's *Esquisse géographique du département de la Somme* (21), and was interested. Meanwhile Lartet, whose own researches in the Dordogne district were beginning to bear fruit, had written in February 1859 to tell Boucher de Perthes that he

¹⁶ II, 273.

¹⁷ II, 362.

¹⁸ *Sous dix Rois*, VII, 295.

¹⁹ *Mém. de la Soc. d'Ém. de la Somme*, Nov. II, 58.

²⁰ Prestwich, p. 119.

²¹ Prestwich in *Proc. Roy. Soc.*, x, 50.

NINETY YEARS AGO

considered their discoveries bore each other out (22), and the academic temperature had grown a trifle warmer.

Prestwich decided not to wait for the summer, but to make an expedition to Abbeville at Easter 1859. He invited a number of his friends of the Geological Society to accompany him, and to suit them postponed the visit until the end of April. John Evans (my father) wrote to his fiancée in March: 'Think of their finding flint axes and arrowheads at Abbeville in conjunction with bones of Elephants and Rhinoceroses 40 feet below the surface in a bed of drift. In this bone cave in Devonshire (23) now being excavated by the Geological Society they say they have found flint arrowheads among the bones, and the same is reported of a cave in Sicily. I can hardly believe it (24)'.

Prestwich reached Abbeville on 27 April. He was then a man of forty-seven, a member of a firm of wine merchants in London: a man who combined great scientific gifts with so kind a heart that he would have found it hard to demolish another man's theory, but for his spontaneous need for expressing the truth that was in him. He was very tall and thin, with a surprisingly booming voice and a pleasant sense of humour. He began by paying a state visit to Boucher de Perthes, who lived in a big town house set round a courtyard entered through an iron grille, with the Société d'Émulation housed in one of the wings, and the office of the excise in the other. The central part of the house was crowded beyond belief with every imaginable antiquity: much fine pottery, many family pictures, and all the old Picard furniture that had never got to the Musée de Cluny. Boucher de Perthes, now past seventy, had 'a great deal of manner'—to use a familiar Prestwichian expression—and a very obvious wig. His evident ignorance of geology, his preoccupation with chimerical flint sculptures, and his avowed preference for literary work, were none of them calculated to inspire confidence in the London merchant who was also a scientific geologist. Yet there, among the untidy heaps of later antiquities, were undoubtedly early flint implements, together with fossil bones that Boucher de Perthes swore he had seen drawn from the same stratum.

Prestwich anxiously awaited the arrival of his friends, but the weather was bad, the crossing stormy, and of all of them only John Evans, a busy paper maker from Hertfordshire, ten years younger than himself, turned up late in the evening. 'We went straight to bed', Evans wrote, 'and soon after 7 the next morning M. Boucher de Perthes, the first discoverer of the stone axes we were in pursuit of, came to take us to some of the gravel pits from whence his collection had been derived. A M. Marotte, the Curator of the Museum [of the Société d'Émulation], accompanied us but we did not succeed in finding anything'. They adjourned to Boucher de Perthes' own collections: the old man had bought a new visitors' book for the occasion, and the English visitors duly signed on the virgin page. The visitors were greatly impressed with the collections, though some of the implements seemed to them suspect, and the sculptures accidental. The too-honest Englishmen told him as much. However, the vast majority of the axes and other weapons were obviously genuine. A message came to Prestwich from M. Finsard, Boucher de Perthes' henchman at Amiens, that an axe had turned up in the Saint-Acheul pit and had been left *in situ* for their inspection. After a sumptuous fork-luncheon they all set off for Amiens, where they were met by Finsard, the President of the Society of Antiquaries of Picardy, and the librarian. 'We proceeded to the pit where sure enough the edge of an axe was visible in an entirely undisturbed bed of

²² Auf-ère, *Préhistoire*, p. 112.

²³ Torquay.

²⁴ Joan Evans, *Time and Chance*, p. 100.

ANTIQUITY

gravel and eleven feet from the surface. We had a photographer with us to take a view of it (25) so as to corroborate our testimony and had only time to get that done and collect some 12 or 15 axes from the workmen in the Pit when we were forced to take the train again to Abbeville'. The next day they went to see the sandpit at Mencheville near Abbeville, which they found much richer in fossils if poorer in flints. They were impressed by the difference in colour between the whitish weapons from Mencheville and the dull ochreous yellow of the implements from Saint-Acheul, and by the realization of the time such a change of colour must have taken. That evening they returned to England, wholly converted.

On the evening of 5 May Prestwich entertained a number of geologists and antiquaries with his legendary sherry and a view of the implements, and they in their turn were greatly interested. Falconer generously gave up any claim to credit in the matter (26), and publication was left to Prestwich and Evans. On the 26th, Prestwich read a paper to the Royal Society (27), which, reported John Evans (28) 'would have been very good but he had only time to give an indifferent abstract of it and his voice was hardly audible in that room . . . I had written an antiquarian letter to him to incorporate in his paper, and this he dexterously managed to leave behind him. The result was I had to stand up and give up an extempore lecture . . . There were a good many geological *nobs* there, Sir C. Lyell, Murchison, Huxley, Morris, Dr Perry, Faraday, Whetstone, Babbage, etc., so I had a distinguished audience. Our assertions as to the finding of the weapons seemed to be believed'.

Belief, indeed, became possible when it was an experienced geologist, and not Boucher de Perthes, who presented the case. Prestwich described the stratification he had found at Saint-Acheul. Below the stratum of brown brick earth and flint gravel, came a second stratum of whitish marl and sand with small chalk debris and freshwater shells. Beneath this lay a bed of coarse subangular flint gravel, white with irregular, ochreous and ferruginous seams, with tertiary flint pebbles and small sandstone blocks. In this layer were found teeth and bones of elephant, horse, ox and deer, and the worked flints that Boucher de Perthes calls *haches* and the workmen *langues de chat*. Prestwich—explaining as politely as he could Boucher's failure to get his discoveries accepted—ascribed it to the indifference of his illustrations and the inclusion of 'doubtful forms of an accidental kind'. John Evans, in his brief contribution at the end of the paper, classified the implements: arrowheads, knives, and pointed, truncated and almond-shaped axes. It is noteworthy that even to so learned an audience he had to explain that the flints from Saint-Acheul were not those generally called Celtic—that is neolithic—and that even the 'Celtic' were not the handiwork of the Celts. (It was not until 1865 that Sir John Lubbock invented the convenient words palaeolithic and neolithic (29).

Three days after the paper on 29 May, 1859, Prestwich returned to Abbeville with Godwin-Austen, Flower and Mylne for further verification. They opened trenches in virgin ground and themselves found fossil bones and worked flints at a considerable depth. On 23 June Boucher de Perthes gratified the Société d'Émulation with letters from

²⁵ Many years ago I remember being shown a print of this, but have failed to find it now, in spite of the search kindly made by Mr Leeds and Mr Harden among the photographs left to the Ashmolean Museum by my brother Sir Arthur Evans.

²⁶ Prestwich, p. 123.

²⁷ Reported in *Proc. Roy. Soc.*, x, 50.

²⁸ *Time and Chance*, p. 103.

²⁹ *Prehistoric Times*, I, 3.

NINETY YEARS AGO

Prestwich in which the authenticity of his discoveries of implements in the Drift was handsomely confirmed.

On 2 June John Evans read his own account of the discovery to the Society of Antiquaries (30). He had found in the volume of *Archaeologia* for 1800 a paper by Frere on flint implements found at Hoxne in Suffolk in company with fossil bones; he had induced Prestwich to examine the site, and in October they employed men to dig trenches in the gravel pits and Evans discovered half an axe in a similar stratum to the axe-bearing layer at Saint-Acheul.

Meanwhile in August Sir Charles Lyell himself came to see the Abbeville pits, was convinced in his turn, and on 18 September reported on the whole question to the British Association at Aberdeen, in the presence of Prince Albert as President. The battle was won at last. It was an eventful week; the *Great Eastern* had just made its first Atlantic crossing; the men of the building trade had just struck for a nine-hours day; Brunel had died and Tennyson published the *Idylls of the King*. Yet even so the importance of the discoveries and their implications did not go unremarked. On 18 November, the *Times* published a letter from T. W. Flower reporting that he had himself found an axe *in situ* at Saint-Acheul, 18 inches inside the quarry face. On 1 December 'Senex' came into print to declare that the flints are not worked at all, but split by the action of frost and broken by the force of streams; or, if they are worked, they must be antediluvian in the strict biblical sense. It is impossible that man should have existed 14,000 years ago as Horner and Darwin pretend. John Evans answered him two days later, laughing gently at his perplexities and stressing the importance of the Hoxne finds of 1797. On 5 December Senex replied much out of temper and asking what is drift? But he and his like could do nothing against proven fact; and it was the proof of the antiquity of man in 1858 which made the theories of Darwin's *Origin of Species*, published in 1859, directly applicable to *Homo sapiens*.

Boucher de Perthes was thankful for his English friends' backing, but more anxious for his own reputation than for the scientific implications of his discoveries. We may guess that his friends in France had not wholly forgotten Picard. In a letter to Prestwich of 31 May 1859, Boucher claims to have sent axes to the British Archaeological Association 'vers 1842': in fact it was not until seven years later. By the autumn, however, his fame was assured. Prestwich had addressed a letter to the French Academy of Sciences on the importance of Boucher de Perthes' discoveries. In consequence Albert Gaudry went and investigated them and confirmed their validity, and was followed by a procession of those who formerly had not thought it worth their while to come. On 4 November 1859, Hugh Falconer wrote to Prestwich (31):

'I have a charming letter from M. Boucher de Perthes, full of gratitude to *Perfide Albion* for helping him to assured immortality, and giving him a lift when his countrymen of the Institute left him in the gutter. He radiates a benignant smile from his lofty pinnacle on you and me—surprised that the treacherous Leopard should have behaved so well'.

By December, Boucher de Perthes was once more offering his collections, this time to the Museum of Natural History (32), which at least accepted a number of specimens. On 7 June 1860, he made a grand address to the Société d'Émulation (now Impériale) (33) to claim that it was now nearly a quarter of a century since he had revealed to its members

³⁰ See *Archaeologia* xxxviii, 280; *Proc. Soc. Ant.* iv, 329.

³¹ Prestwich, p. 141.

³² *Sous six Rois*, vii, 381; vii, 436.

³³ *Mémoires*, 1860, p. 471.

ANTIQUITY

the extreme antiquity of man and his probable contemporaneity with huge antediluvian animals. By this time he dated his first interest in the subject to 1805, when he had visited the Grotte de Roland near Marseilles and had found bones; and he dated his collections from 1836, with a pleasing vagueness as to the epoch of the objects they included.

The sunshine of his belated glory shone yet more brilliantly in 1862, when he had the chance of speaking to the Emperor of his discoveries, but was clouded in 1863 when his discovery of a human jaw at Moulin Quignon was not accepted by his English friends (34) though welcomed by the Frenchmen who had before been sceptical about his discoveries. By this time the old man was beginning to fail, and the English sceptics hardly like to press too hard their conviction that the Moulin Quignon jaw was modern and the implements found with it fakes; and that the whole find had been 'planted' by the workmen for Boucher de Perthes to discover.

The best things in his collections were now enshrined in the Musée Impérial at Saint Germain, though much still remained in the house at Abbeville, which he had bequeathed to the city to form a permanent museum. The chief occupation of his declining years was the preparation of a work in eight volumes entitled *Sous dix Rois*. It included 1355 letters in which the story of his life was told. In them one may often suspect that the pen of the novelist and dramatist has improved upon the original draft, especially in the passages in which a pre-Picard interest in prehistory is alleged. A letter from Morlaix of August 1823 (35), for example, drags in the question of 'ces haches dites celtiques' and the fact that he has already found several near Abbeville (36). The book was hardly out before Boucher de Perthes died on 2 August 1868, aged eighty. His fame was safe. The *Petit Larousse* does not include Picard, Rigollot, Falconer, Prestwich or Evans, but it duly records 'Boucher de Perthes, Jacques . . . créateur de la science de la préhistoire sur l'homme'.

It was in August 1900 that my father took me to Saint-Acheul. He had often stopped at Amiens to visit the pits on his way to Paris in the years since that momentous visit in 1859; he still had friends of long standing among the workmen. They brought out the season's spoils of yellowed flint, and my father bought three or four implements for me. The workmen in their deep-belted baggy corduroy trousers looked on with the benevolent approval that any Frenchman gives to a family scene, as my father stroked and balanced the implements in his experienced hands and handed them over to my childish ones. Old Monsieur Boucher de Perthes and his flints, and my father and Prestwich and all their excitements and enthusiasms, were ancient history to them, but they were pleased and amused that the old Englishman, whom they liked for his old-fashioned politeness, should turn up with a child of seven. I doubt if any of them—my father included—remembered on that sunny August morning that in that gravel pit a new science of prehistory had had its beginnings. I at least had the sense of being initiated into the mystery of my father's lore; I treasure those flint implements to this day.

³⁴ See *Time and Chance*, p. 116.

³⁵ IV, 309.

³⁶ Another rather suspect passage occurs in a letter to Becquerel, 14 March 1838, v, 185. See also v, 548 and 558.

NINETY YEARS AGO

BIBLIOGRAPHY

- L. AUFRÈRE. Essai sur les premières découvertes de Boucher de Perthes et les origines de l'archéologie primitive. Paris, 1936. (*Épreuves et Synthèses*, no. 1).
- . Figures de préhistoriens, I. Boucher de Perthes. Paris 1940 (*Préhistoire* VII).
- J. BOUCHER [de Crèvecœur] de Perthes. *Antiquités celtiques et antédiluviennes*. I. Paris 1847, II. Paris 1857.
- . Antiquités antédiluviennes. Réponse à MM. les Antiquaires et Géologues présents aux assises archéologiques de Laon. Amiens 1859.
- . L'homme antédiluvien et ses œuvres. In *Mém. de la société impériale d'Émulation d'Abbeville*, II. Abbeville 1861, p. 471.
- . *Les Masques : biographie sans nom. Portraits de mes connaissances dédiés à mes amis*. Paris 1861. 2 vols.
- . *Sous dix Rois : Souvenirs de 1791 à 1860*. Paris 1863-68. 8 vols.
- JOHN EVANS. On the occurrence of Flint Implements in undisturbed Beds of Gravel, Sand and Clay, in *Archaeologia* XXXVIII, 280.
- A. LEDIEU. *Boucher de Perthes, Sa vie, ses œuvres, sa correspondance*. Abbeville, 1885.
- E. LITTRÉ. Études d'histoire primitive, in *Revue des Deux Mondes*, XXVIII (2nd series XIV), 1858, p. 1.
- G. A. MANTELL. On the remains of man and works of art imbedded in rock and strata as allusive of the connexion between archaeology and geology. Privately printed 1857.
- H. and J. PERCHELLET. *Correspondance de J. Boucher de Perthes (1788-1868). Témoin de dix règnes*. Le Raincy, n.d. (1947).
- A. G. PRESTWICH. *Life and Letters of Sir Joseph Prestwich*. Edinburgh and London 1899.
- J. PRESTWICH. On the occurrence of Flint implements, associated with the remains of extinct mammalia, in Undisturbed Beds of a late Geological Period, in *Proc. Royal Soc. of London*, X, 1859-60, p. 5.
- DR RIGOLLOT. Mémoire sur des instruments en silex trouvés à Saint Acheul près d'Amiens, in *Mém. de la Soc. des Antiquaires de Picardie*, IV, 1856, p. 23.

Human Evolution: a review

by E. A. HOOTON*

FOR more than half a century Sir Arthur Keith has been one of the world's leading authorities on the physical evolution of man. As a young man, Sir Arthur spent some years of medical practice in Borneo and became interested in the comparative anatomy of the anthropoid apes and the relation of these higher primates to man. By dissections and other anatomical studies he accumulated the largest body of accurate scientific information on this subject available to students before Professor Adolph H. Schultz began his researches in this same field in the nineteen twenties. In 1901 Sir Arthur published his *Human Embryology and Morphology*, which is one of the few technical treatises on man's embryology that is anthropological and primatological in its orientation. From 1908 onward Keith concerned himself with the problem of the transformation of the body and brain of the ape precursor into that of a human being. This subject led him into intensive studies of the skeletal remains of fossil men and apes, culminating in his great works, *The Antiquity of Man* (1925) and *New Discoveries Relating to the Antiquity of Man* (1931).

During the long years in which he was thus engaged, Sir Arthur was also considering the evolution and differentiation of the modern races of man, not only from the biological and anatomical viewpoints, but also on the bases of history, social anthropology, and psychology. Recently, as he approaches the end of his fruitful scientific career, this great anthropologist has produced two volumes dealing with human evolution in its entirety—a preliminary book *Essays on Human Evolution* (1946), and the present work.

The study of anthropology has diversified into so many specialities that it is now well-nigh impossible for any single individual to keep abreast of all of the advances in the numerous fields: physical anthropology, archaeology, technology, social anthropology, psychology, linguistics, etc. The specialists in these various subdivisions, all of whom may call themselves anthropologists, often speak in tongues that are hardly mutually intelligible; they often represent opposing schools of thought and interpretation (e.g. heredity versus environment). The anthropologists who study culture sometimes pay very little attention to human biology and *vice versa*. Unfortunate antagonisms have developed between these diverse classes of students of man. It is hardly possible, then, for the anthropologist whose orientation is primarily biological to produce a general work on human evolution that will be received with approbation by cultural anthropologists. Sir Arthur Keith undoubtedly recognized these difficulties and hazards before he committed himself to such an undertaking. However, he has always been a courageous and independent scientist, ready to express his opinions and convictions whenever he considers them to be substantiated by valid evidence, quite irrespective of their conformity, or lack of it, to current scientific opinion and popular prejudice.

In *A New Theory of Human Evolution*, the author begins by summarizing his 'new or group theory'. His thesis is that evolution has taken place mainly through the

* *A New Theory of Human Evolution*. By Sir Arthur Keith. London, Watts & Co., 1948, pp. vii, 451. 21s.

HUMAN EVOLUTION

struggle for survival of small, local, inbreeding groups ('evolutionary units'), which were in existence from the earliest times when the development of the human brain made speech possible and right down to the dawn of civilization. Tribes (large, isolated, inbreeding evolutionary units) represent a second stage of the process, and relatively late in human development these tribal units are converted into nations. So far, there is nothing in Sir Arthur Keith's presentation that can be called 'subversive'. However, he holds that two opposing aspects of man's nature 'must be balanced to secure continuous, progressive evolutionary changes'. These are the 'code of amity' which favours the growth and exercise of all of the generally recognized Christian virtues, and the 'code of enmity' which encourages antagonism, suspicion, distrust and contempt of competing evolutionary groups and hostility toward them. It is this stress upon the continuing importance in human evolution of the qualities that are 'condemned by all civilized minds' ('emulation, envy, the competitive spirit, deceit, hate, anger, ferocity, and enmity') that is unlikely to commend Sir Arthur Keith's 'new theory of human evolution' to idealistic and internationally-minded scientists and laymen.

In detail and successively, Sir Arthur documents his theory with the citation of opinions of authorities on the nature of original evolutionary units, with synopses of the evidence of the existence of such units among the Primates and in the various human groups, with a discussion of the importance of the ownership of territory as a factor in human evolution, of group spirit, patriotism, cooperation and competition, mental bias, resentment and revenge, and the search for status (drive for superiority). He proceeds to discuss human nature as it is, rather than as it ought to be, and the effects upon evolution of leadership, loyalty, and morality.

The next portion of the book deals with the machinery of evolution; the genes, the rôles of isolation and inbreeding (endogamy, exogamy, monogamy); sex differentiation and the sex hormones; sexual and racial selection, and foetalization. In this section Sir Arthur handles subjects in which he is a recognized authority. There follows a most interesting and important exposition of the spread of the anthropoidal ancestors of mankind and of the differentiation of modern man into five major physical groupings, based upon development in five major areas of the Old World. Keith postulates Africa as the original cradle of humanity and assumes that the 'Dartians' (the man-apes or ape men recently discovered in fossil deposits in South Africa) were responsible for carrying abroad the genes that gave rise to the various human types subsequently differentiated in the several major areas. He then discusses the relation of types of fossil men to modern races. He believes that the separation of modern races is very ancient—traceable to the beginning of the Pleistocene—and that each evolved in its own area. Many resemblances between extant human races are due to 'parallel evolution' or 'convergence'. The issues dealt with in this part of the book are highly debatable. Perhaps most interesting to the physical anthropologist is Sir Arthur's abandonment of his position as the champion of the early Pleistocene existence of anatomically modern man (*Homo sapiens*) and his acceptance of the belief held by Hrdlicka, Weidenreich, and others that modern man is a direct descendant of such apelike Pleistocene human forms as Neanderthal man. This apostasy comes at a time when the discovery of Swanscombe man and the very recent find of Fontéchevade man (the latter subsequent to Sir Arthur's book) seem finally to confirm the correctness of his earlier view, although the researches of Ashley-Montagu and Oakley last summer have probably disposed of the claims of Galley Hill man to high antiquity, in agreement with Keith's latest opinion.

The next sections of the book discuss the development of civilization; hunting, agriculture, village communities and the transformation of the latter into city units

ANTIQUITY

and city states in Mesopotamia and Egypt. Then comes an exposition of the author's ideas on the evolution of European nationalities from local evolutionary units, exemplified by Scotland, together with a discussion of the classification of the races and peoples of Europe. Here is a most interesting heresy on the part of Keith. He proposes to revert to the earlier use of the term 'race' as applied to small, local, inbreeding groups and to abandon the current anthropological practice of applying the term only to a major physical grouping of mankind, based upon the possession of a distinctive combination of inherited anatomical features. Sir Arthur regards the smaller local groups as essentially the 'race-makers' and proposes to use the term 'variety' for the larger differentiated physical group. All the world except the anthropologists use the word 'race' to apply to groups that are territorial, linguistic, political, and cultural, and Keith's proposal is a reversion to a substantially similar, but better defined usage, of the term. I suspect that this suggestion will not be acclaimed among anthropologists.

The final essays of the book deal with nationalism, racialism in South Africa, national self-determination as illustrated by the case of the Irish Free State, the Jews 'as a nation and as a race', nation-building in the United States, the rise of nations in the British Dominions, and, finally, the somewhat dim prospect of an evolutionary future of man in which competition between nations will be suppressed by mutual agreement and peace will reign.

Nearly all of these subjects are very hot potatoes. Sir Arthur grasps them fearlessly and firmly; he certainly does not juggle with them, but I doubt that his handling and his serving out of portions will satisfy the palates and appetites of all physical anthropologists and of more than a few cultural anthropologists and practitioners of the 'social sciences' in general. There is a grim realism (mistakenly condemned as 'pessimism') which recognizes that men are still ruthlessly competitive brutes, and that associations of human beings, such as nations, cannot easily be converted to altruistic attitudes and humanitarian endeavours, in spite of our individual ideals and aspirations. Any one who presents these unpleasant views is sure to be accorded the traditional reception of the bearer of bad tidings.

Sir Arthur Keith himself is one of the most kindly and tolerant humanitarians whom this reviewer has ever been privileged to know. He is a great biological anthropologist and his mature reflections on human evolution should be read with care and respect by all persons interested in the present evolutionary state of man and in his future prospects. They should not be dismissed with scorn and contempt because they are out of line with roseate vistas of an era of universal fellowship of man which we may like to think is just around the corner. We are likely to turn a great many corners and be confronted with many ugly and depressing prospects before we arrive at this golden goal (if we ever do).

There are many points of fact and theory in *A New Theory of Human Evolution* that this reviewer would like to discuss and debate with Sir Arthur and with the readers of ANTIQUITY. Space forbids, and, in any event, I do not believe that the proper function of a reviewer is to inject his own opinions and theories into what should be an unbiased summary of another man's work. I will say merely that I think no anthropologist should neglect to read this book and I hope that each of its readers will display the tolerance of dissenting opinion and the readiness to recognize the errors of his own positions, and to revise them, that has always characterized Sir Arthur Keith.

The Origin of Neolithic Culture in Northern Europe

by V. G. CHILDE

TILL 1948 the coherent record of farming in Northern Europe began with the neolithic culture represented in the Danish *dysser* ('dolmens') and most readily defined by the funnel-necked beakers, collared flasks and 'amphorae' found therein. As early as 1910 Gustav Kossinna (1) had remarked that these distinctive ceramic types, and accordingly the culture they defined, were not confined to the West Baltic coastlands, but recurred in the valleys of the Upper Vistula and Oder to the east, to the south as far as the Upper Elbe and in northwest Germany and Holland too. He saw in this distribution evidence for the first expansion of *Urindogermanen* from their cradle in the Cimbrian peninsula. In the sequel Åberg (2) filled in the documentation of this expansion with fresh spots on the distribution map and Kossinna himself (3) distinguished typologically four main provinces or geographical groups—the Northern, Eastern, Southern and Western. Finally Jazdrzewski (4) gave a standard account of the whole content of what had come to be called *Kultura puharów lejkwatych*, *Trichterbecherkultur*, or *Tragtbaegerkulturen*. As 'Funnel-necked-beaker culture' is a clumsy expression and English terminology is already overloaded with 'beakers', I shall use the term 'First Northern'.

The origin of this vigorous and expansive group of cultivators and herdsmen has always been an enigma. Not even Kossinna imagined that the savages of the Ertebølle shell-mounds spontaneously began cultivating cereals and breeding sheep in Denmark. As *dysser* were regarded as megalithic tombs and as megaliths are Atlantic phenomena, he supposed that the bases of the neolithic economy were introduced from the West together with the 'megalithic idea'. But the First Northern Farmers of the South and East groups did not build megalithic tombs. Moreover, in the last ten years an extension of the North group across southern Sweden as far as Södermannland (5) has come to light, and these farmers too, though they used collared flasks and funnel-necked beakers, built no dolmens either. In any case there was nothing Western about the pottery from the Danish *dysser*, and Western types of arrow-head are conspicuously rare in Denmark (6).

Becker's recent study (7) has put the whole question in a new light. It began with a re-examination of pottery recovered from Danish peat-bogs. It was a practice of the

¹ 'Ursprung und Verbreitung der Urfinnen und der Urindogermanen', *Mannus*, I.

² *Das nordische Kulturgebiet in Mitteleuropa*, 1918.

³ 'Entwicklung und Verbreitung der steinzeitlichen Trichterbecher . . .', *Mannus*, XIII, 1921.

⁴ 'Kultura Pucharów Lejkowatych w Polsce' (*Bibliotheca Prehist.*, 2), Poznań, 1936.

⁵ Bagge and Kjellmark, *Stenåldersboplatserna vid Siretorp i Blekinge*, Stockholm, 1939; Florin, 'Vrå Kulturen', *Kulturhistoriska Studier tillägnade Nils Åberg*, Stockholm, 1938.

⁶ Clark, *Man*.

⁷ C. J. Becker, 'Mosefundne Lerkar fra yngre Stenalder: Studier over Tragtbaegerkulturen i Danmark', *Aarbøger*, 1948 (also published separately), 318 pp. Danish text, xviii pp. English summary, xxviii plates, København, 1948.

ANTIQUITY

Early Neolithic Danes to deposit in peat-bogs complete pots and other articles, presumably as votive offerings. (Incidentally other groups of First Northern farmers observed the same custom and its roots may be traced back to the early Mesolithic and Upper Palaeolithic offerings cast into meres at Meiendorf and Stellmoor (8)). As a consequence Danish museums contain large numbers of such vessels, and the scientific examination of Aamose has added new vases, some pollen-dated. These made it desirable to re-examine the whole assemblage and compare it with vessels from tombs and settlements that can be placed in the recognized sequence of neolithic cultures.

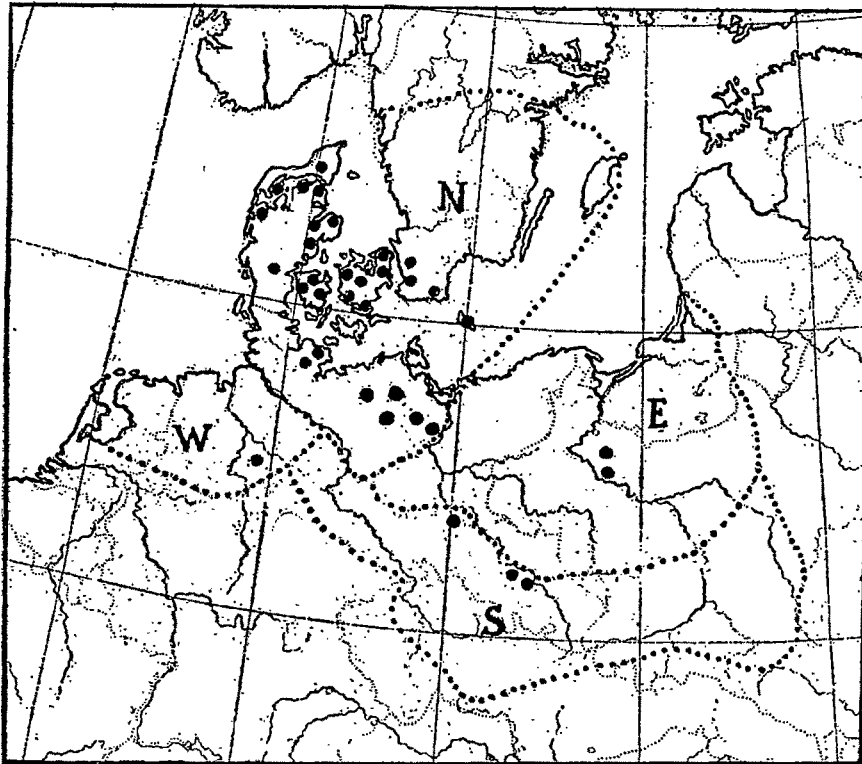


FIG. 1. DISTRIBUTION OF FIRST NORTHERN VASES OF TYPES A AND B
The approximate boundaries of the four provinces in Phase C are indicated by dotted lines

Becker has been able to work out a typological series of four stages, best exemplified in the funnel-necked beakers but supported by the associated types. The general direction of the evolution is adequately established by the associations of the later stages. Beakers of stage D occur in early Middle Neolithic (Passage Grave) sites like Trøldbjerg, those of stage C in *dysser* and contemporary earth-graves. Stages B and A should therefore be older than the first *dysser*. B types are represented in the settlement at Havnelev in Zealand, in a celebrated shaft-grave at Virring in Jutland—which Brøndsted had claimed ten years ago as 'pre-Dolmen'—and in certain shell-mounds.

⁸ Rust, *Die altsteinzeitliche Renntierjägerlager Meiendorf*.

⁹ *Wiadomosc Archeolog.*, xv, 1938, 1-105.

THE ORIGIN OF NEOLITHIC CULTURE IN NORTHERN EUROPE

Becker is then left with a small group of curious flat-bottomed vases—eleven beakers and two amphorae—which, as they already contain casts of grain, must rank as the products of the earliest farmers to clear land in Denmark. No evidence is published to prove the priority of A over B types—indeed in the Solager shell-mound both occur together in the same layer (II) though below C types—but pollen analysis may establish it. In any case A and B are certainly pre-megalithic. The A types are even less Western than the C vases

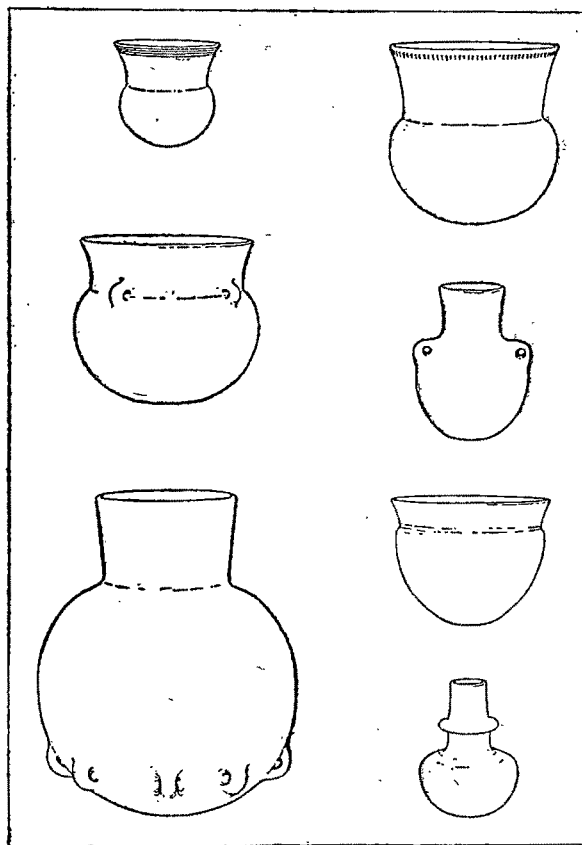


FIG. 2. TYPE OF FIRST NORTHERN POTTERY FROM DENMARK
after Becker

from *dyser*. Though Becker is inclined to admit Western, 'Michelsberg' influence to account for the rounded bases of B types, he feels obliged to look elsewhere for the origin of the first Danish farmers.

Applying this result to other regions, Becker finds B types in southern Sweden and what he calls A/B types of beaker and amphora in northern Germany and between the Oder and the Vistula as far south as Wrocław. The latter already occupy the East and South provinces of the First Northern culture as defined by Jazdrzewski and Kossinna. They are never associated with the assemblages described there by these authors, but

ANTIQUITY

are at least typologically anterior thereto. They represent prototypes from which the mature East and South groups might have developed locally just as the North group did in Denmark. A new discovery described by Becker shows this local development



FIG. 3. PLANS OF HOUSES AT BRZESC KUJAWSKI, POLAND

was more than a mere possibility, for it establishes the true chronological relation between the East group and the North group.

An earth-grave at Salten near Silkeborg, Jutland, assignable by its form and by the arrow-heads, axes and beads it contained to stage c, contained also a small embossed disc of copper (according to a footnote analysis implies that it was made of smelted copper).

THE ORIGIN OF NEOLITHIC CULTURE IN NORTHERN EUROPE

This was certainly not made in Denmark but imported. An almost exact parallel comes from a grave at Brzesc Kujawski (between Warszawa and Poznan) which can in turn be shown to be contemporary with the mature East group of funnel-necked beakers. The

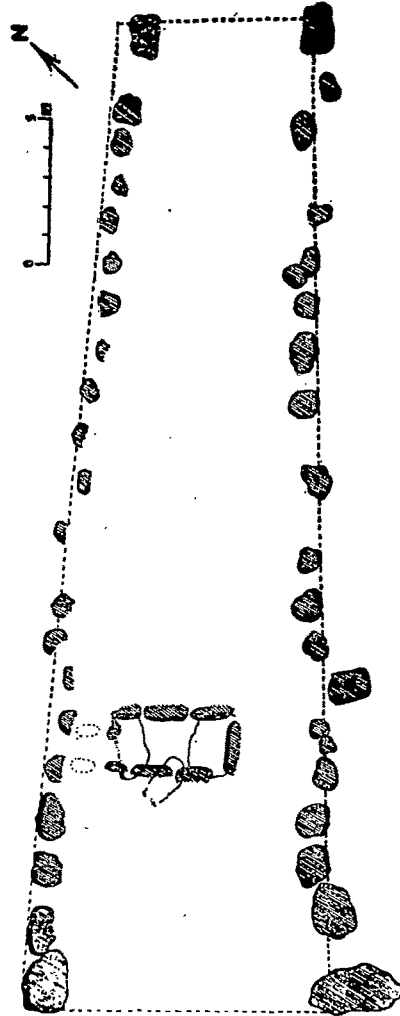


FIG. 4. PLAN OF LONG BARROW
AT DWASIEDEN, RÜGEN

latter therefore cannot be derived from the Northern group by a slow migration; an actual interchange of trade goods shows it rather to be contemporary with the Northern group in its phase C. An equal parallelism in time may be assumed between the earlier A-B phase of both groups. Indeed these early continental vases may have been made by the ancestors of the first farmers who appear as immigrants in Denmark, and the origin of the First Northern culture must be examined from this new stand-point.

ANTIQUITY

The pot forms still fail to indicate kinship with any known neolithic group. So Becker looks to some undefined cradle in the east whence the whole culture might have come fully formed. But his discussion of the Danish *dysser* helps to clear the way for a more economical hypothesis. Using the results of Thorvaldsen's article on 'Grave-finds from the Dolmen period' (10) Becker in effect shows that at the beginning of phase C the First Northern Farmers in Denmark did not bury their dead in collective megalithic tombs. A third of the tomb-groups of this phase in fact come from earth-graves containing a single extended skeleton laid on the ground within a ring of stones, perhaps protected by wooden planks and probably always covered by a low but elongated barrow (that at Salten was 19.5 m. long, the grave lying 5 m. from the east end). The earliest *dysser* themselves were completely closed stone boxes, built indeed of huge erratics but too small to be family vaults. Of 14 measurable early examples ten were less than 2 m. long inside. Eight stood in a long rectangular mound defined by a peristalith of big boulders but never near the edge so as to be accessible from outside. Functionally such *dysser* are quite different from the classical megalithic tombs of Atlantic Europe. They are single graves, designed to hold one extended skeleton. Now individual burial in the extended attitude, sometimes in cists of thin slabs, was the normal burial rite also in the East and South groups. On the moraines of Denmark erratics provide the only stone readily accessible for building, but it cannot easily be split up into slabs. The *dyss* is thus a local version, conditioned by available material, of the type of sepulchre common to all First Northern groups. Extended burial is in fact one of the distinctive traits of that culture (11).

But burial in the extended attitude, contrasting with the crouched or flexed position approved by most neolithic societies, had been practised already in mesolithic times in the North (12). Now other traits link the First Northern barbarians with the savages who had hunted and fished on the forested plain of Northern Europe before them—a preference for flint as the material for axes, transverse arrow-heads, battle-axes (for the polygonal stone battle-axes, current among all groups by phase C, must be derived in the last resort from antler axes such as were used by Ertebølle people in Denmark and still buried in graves at Brzesc Kujawski), an affection for amber, votive offerings in bogs. Hence even in 1946 (13) I had hinted that the First Northern culture might be due to the acculturation of mesolithic survivors. But such acculturation could not have taken place in Denmark. There the First Northern Farmers were the immigrants, sharply contrasted to the hunter-fishers who were still adding to the celebrated Ertebølle shell-mounds. And no other immigrants have been detected (the 'corded ware' of Virring and Vrå turns out to be only a variant of the local First Northern pottery in its phase B).

But between the Oder and the Vistula Danubian peasants had spread beyond the löss onto the once glaciated plain and had even reached the Baltic coasts. Precisely here we now find an early stage of the First Northern culture represented. Here then—presuming a survival of mesolithic Forest folk during Atlantic times—hunter-fishers could have acquired grains and sheep and learned to make pots from the newly arrived Danubian farmers. Of course they would not imitate their masters' gourd-shaped pots, but would

¹⁰ 'Dyssetidens Gravfund i Danmark', *Aarbøger*, 1941.

¹¹ Jazdzewski, *Kultura Pucharów*, 336.

¹² Fresh well-attested Ertebølle burials are reported in *Fra Nationalmuseums Arbejdsmark*, 1945, p. 6, and Mathiassen, 'En Boplads fra ældre Stenalder ved Vedbaek Bolbaner', *Søllerød Bogen*, 1946.

¹³ *Dawn of European Civilization*, 4th edit., 176.

THE ORIGIN OF NEOLITHIC CULTURE IN NORTHERN EUROPE

reproduce in the new medium the old skin and bladder vessels, and perhaps they would further degrade the predatory Danubian rural economy. So, practising the most wasteful form of *jhuming* cultivation, they would spread quite fast and soon reach Denmark where the peat bogs reflect just such a transitory occupation of the land—clearance by fire of a patch of forest which was then allowed to regenerate (14).

As a topical appendix I might mention a curious circumstance. The Danubian peasants lived in very long houses, some rectangular others trapezoid in plan. Some at Brzesc Kujawski (FIG. 3) were as much as 32 m. long, 10 m. wide at the south end but only 5 m. at the inner extremity. Now some First Northern farmers in the East and South groups laid out the long barrows over their graves on a very similar plan. A couple of Kujavish graves in Poznań (15) of phase c were 80 m. long and almost triangular in plan. Some tombs in Rügen (FIG. 4), of rather uncertain age, are strictly trapeze-shaped. That at Dwasieden (16) was 34 m. long, 6 m. wide at one end and 11 m. at the other and contained a small chamber near, but not at, the wide end. In the South group the Pohlberg near Latdorf was again a trapeze shaped barrow, 25 m. long covering a cist only 1.4 m. in length. It is tempting to see in this curious plan an attempt to make the house of the dead approximate to habitations such as are illustrated at Brzesc Kujawski. Sprockhoff (17) indeed has pointed out that in its original form a North German long barrow (*Langdolmen*) would look very like a house with the wall timbers represented by stones and the thatched-gabled roof by turf. Can similar ideas have influenced the builders of our long barrows? In any case, Prof. Glob, in describing the early Neolithic village of Barkaer in Jutland (18), has just compared the long rows of contiguous rectangular houses there exposed to the Danish *langdysser*—long rectangular stone settings enclosing one or more megalithic cists ('dolmens'); he similarly compares the *runddysser*—one 'dolmen' surrounded by a circular peristalith—to the round houses found, side by side with the long Barkaer type, by Winther at the Middle Neolithic site of Trøldbjerg on Langeland.

NOTE. Owing to the author's absence abroad he has not been able to correct proofs, and one or two references remain incomplete.—EDITOR.

¹⁴ Iversen, 'Landnam i Danmarks Stenalder', *Dansk Geolog. Undersøgelse*, II R., no. 66, 1941; summarized in *ANTIQUITY*, XIX, 1945, 61.

¹⁵ Jazdrzewski, op. cit. 383 and fig. 980.

¹⁶ Sprockhoff *Die nordische Megalithkultur*, 1938, 31.

¹⁷ op. cit. 10, 'Das Grab erschiene wie ein niedriges Haus mit flachem Firstdach'.

¹⁸ P. V. Glob, 'Barkaer', *Fra Nationalmuseums Arbejdsmark*, 194-.

Peru before Pizarro

by G. H. S. BUSHNELL

TWO notable events in Peruvian archaeology, which are of wide general interest, have recently taken place. One was the discovery of pre-ceramic horizons on the Peruvian coast, and the other was a conference held in New York in July 1947, at which several acknowledged experts felt that the time had come to explain the known facts in terms of a general scheme of development, and attempted to do so independently with strikingly similar results. The papers read at the conference on this and other matters have recently been published (1), and they include the fullest summary so far available of the preceramic discoveries.

Mr Junius Bird has long been known for his work on the prehistory of unpromising and difficult regions in South America. He has studied successions in Tierra del Fuego and on the southern end of the Chilean mainland, which contain stone and bone artifacts but no pottery, from which he estimates, by such methods as the rate of rise of land and of accumulation of deposits, that human occupation began about 5000 years ago, i.e. at the beginning of the 3rd millennium B.C. (2). His methods of course involve some very large assumptions, but the results are reasonable when considered in relation to the usual estimate of about 10,000 years for Folsom man. He has also discovered non-pottery and non-agricultural horizons in the middens of the north part of the coast of Chile, but these may not be of any great age and their poverty may be due to the inhospitable nature of the region (3).

His latest work in Peru has been done in the Chicama and Virú valleys on the north coast, but publication is so far limited to what he calls a tentative résumé because the collections have not yet arrived in the United States for detailed study. The most notable result was the discovery of some 40 feet of pre-ceramic midden deposit in a mound called the Huaca Prieta in the Chicama Valley, containing cultivated cotton, gourds, squash, peppers and beans, besides remains of wild food plants and a variety of animal foods, chiefly derived from the sea. An important point is that there is no maize, a blow to the theory that the development of agriculture and civilization in America rests on this plant alone. Cotton textiles, mainly twined but some woven, were found, also fish nets and twined baskets. Bark cloth was made. Gourds were used as containers and floats for fish nets. Coarse stone flakes, cores, hammerstones and net weights were found, but there was no pressure flaking of stone. Subterranean houses of one or two rooms, which occur in the upper part of the deposit, apparently belong to the period. They lack hearths and cooking was done with firestones. The author acutely calls attention

¹ *A Reappraisal of Peruvian Archaeology*. Assembled by W. C. Bennett, pp. ix, 128, 72 figs., 6 tables. *Memoirs of the Society for American Research*, no. 4, 1948. (Supplement to *American Antiquity*, vol. XIII, no. 4, Part 2, April, 1948), Menasha, Wis.

² Bird, J. B., in *Handbook of S. American Indians*, vol. 1, pp. 17 ff (Bureau of American Ethnology, Bull. 143).

³ Bird, J. B., *ibid.*, vol. II, p. 587 ff.

PERU BEFORE PIZARRO

to what our view of the culture of these people would have been, if nothing but their stonework had survived.

A junction between this and a pottery-bearing culture was obtained not in but near this mound, and again near Guañape in the Virú valley, though little perishable material was preserved at the latter site. The first pottery at both places was a generally similar plain ware, and it appears that the succession near Huaca Prieta passes up conformably, in geological language, from this to the more familiar Cupisnique or Coastal Chavin horizon at the base of the previously known succession.

In the Virú valley Bird found a soil profile which seems to show continuous aggradation from the deposition of coarse gravels associated with the recession of the ice in the Andes until quite recent times, when erosion set in. Here Cupisnique sherds were found 6 feet above the base of a 21-foot section, whence the author assumes that the events between the end of the last glacial period and Cupisnique times, including the pre-ceramic occupation, must have lasted only about two-fifths of the time occupied by the previously known pottery-making periods. By methods similar to those he employed further south he estimates that agriculture was established in this region about 5000 years ago, at roughly the same time, incidentally, as he believes human occupation to have begun in southern Chile. This, however, puts the introduction of Cupisnique pottery at about 1800 B.C., instead of about the beginning of the Christian era, as most other people guess. Kroeber, in summing up the results of the whole conference, notes the discrepancy, but believes that the two estimates at least provide us with outside limits.

In addition to the pre-ceramic agricultural remains, Bird and others have found several workshop sites, with abundant pressure-flaked tools but no sherds or camp refuse, in the same neighbourhood. The pressure-flaking shows that these belong to a different culture, which he tentatively ascribes to an older nomadic hunting people, but it seems that a good deal more work will be necessary before this is proved, and experience in Ecuador suggests to the writer that later pottery-making peoples who used stone implements may perhaps have prepared them on such sites. It seems also that Bird may have difficulty in fitting a pre-agricultural period into his 6 feet of post-glacial but pre-ceramic deposit in the Virú valley, so if such a period exists in this region it may indicate, as we suspect on other grounds, that this section is not as complete as it appears to be, which would invalidate his age estimates. Further developments in this connexion will be awaited with great interest.

Turning to the other main theme of the Conference, which we may call, in plain terms, giving meaning to Peruvian archaeology, there are contributions from W. C. Bennett, Gordon Willey and Duncan Strong, all of whom have been active in the field in Peru and have an intimate knowledge of artifacts and styles, and J. H. Steward, whose work of editing the great 'Handbook of South American Indians' (4) must have given him an unrivalled grasp of what is known of economics and social, religious and political organization. There are minor differences in their explanations, particularly in terminology, but the measure of agreement is striking, and an article giving a very similar interpretation for Middle America is contributed by a distinguished Mexican archaeologist, Pedro Armillas.

The pre-agricultural period, whose existence in Peru is still speculative, and the early farmers have already been discussed and do not call for further comment here. Then comes the Chavin culture, called Cupisnique on the north coast, to which the

⁴ Four volumes of the Handbook have been published so far, see note 2.

ANTIQUITY

different authors apply the terms Inter-Areal Developmental, Cultists and Formative (early), in which the communities centred in temples, the art showed a strong religious bias with general similarities over a wide area, and there were rapid technological developments. It is inferred that there were widespread peaceful contacts between communities which were predominantly theocratic.

Next come a variety of cultures, known to specialists as Salinar, White-on-Red, Cavernas, Interlocking, Gallinazo and others, which the authors class as Regional Developmental, Experimenters and Formative (later). Technical developments in agriculture and manufactures approach their limits, and the rise of local styles foreshadows the intense regionalism which was shortly to grow up.

Florescent, Regional Florescent and Master Craftsmen are the terms applied to the succeeding stage, which includes the well known cultures of Mochica, Nazca, Recuay and part of Tiahuanaco. The word 'Florescent' perhaps requires some explanation; it is meant to describe a period in which a number of regions, each sharply differentiated from its neighbours but standardized within itself, reached their highest artistic level. Mochica pottery, with its vast variety of forms and painted scenes, all of them easily recognized as Mochica, provides an admirable and well-known example. The great temple mounds and the ritual scenes on the pottery show that the theocratic state was still dominant, but side by side with this we find representations of warriors, captives and trophy heads, which indicate the rise of a military hierarchy, possibly connected with the increase of population beyond the optimum in each region.

After this, some writers recognize a somewhat obscure epoch of Expansion or Fusion perhaps marked by military conquest, certainly by unrest, associated with the widespread development in the coast of Tiahuanaco-derived, Black-White-Red, and less known pottery styles, which obliterate the strongly regional styles of the preceding stage. Finally all agree in postulating a period of Empire and Conquest, marked first by the Chimu Kingdom on the coast and then by the well-known Inca Empire to which it succumbed, whose rise is a matter of history. These are associated with the construction of urban centres and fortifications, which rival or surpass the temple mounds.

It is not necessary to notice in detail the parallel interpretation of events in Middle America given by Sr. Armillas, but it is worth noting that he minimizes the effect of movements of population on the development of the militaristic stage, ascribing it rather to internal economic crises. Kroeber in his summing up calls attention to a point of great importance, namely the degree of contact between Peru and Middle America. Are the apparently parallel developments in the two areas real, or the result of wishful thinking? If they are real, are they contemporaneous, and to what extent did one influence the other? As regards material products, differences between them are far easier to find than similarities, a point which is underlined by Donald Collier's paper at the same conference, in which he points out the almost complete lack of specific parallels between Peru and the neighbouring republic of Ecuador.

In both Peru and Mexico the Imperialistic or Militaristic stage was abruptly ended by the European invasion. It is interesting but unprofitable to speculate about what the next development would have been if this had not happened, but the study of what went before is of interest to everyone. The isolated position of Peru, and its development apparently undisturbed by outside influences, are in marked contrast with conditions in the Old World, where it would be hard to find a self-contained region which developed naturally without interference from its neighbours. If the natural sequence is from lowly agricultural communities, through small theocratic, peaceful states to great militaristic empires, then Peru provides an ideal example for study, as well as a warning. Such a

PERU BEFORE PIZARRO

general scheme of development is no new thing, as Kroeber points out in his summing up (5), but it has not been applied before to the New World.

Of the other papers read at the Conference, most are of interest mainly to Peruvian specialists. A summary of the Racial History of the Peruvian Area by Marshall T. Newman may be of rather melancholy interest to physical anthropologists, chiefly as showing the nakedness of the land in their particular field. An account by George Kubler, which he repeated in French in a masterly fashion at the 28th International Congress of Americanists in Paris, of an attempt to obtain an absolute dating for the Mochica culture, is of interest as a somewhat bizarre application of geochronology. The method is based on the depth at which objects were found buried in the guano, and depends on the assumption that it was deposited at a constant rate. This implies a constant bird population on the Guano Islands, and anyone who knows that region will readily believe that as many birds as there was room for were always accustomed to sit on the islands! Owing to the destruction of the guano, the results can never be checked, but the approximation obtained, the 7th to 11th centuries, agrees well enough with the guesses of most archaeologists.

⁵ Toynbee, A. J., *A Study of History*, 6 vols., 1933-9, London.

Customs and Traditions of the Isle of Portland Dorset

by ROBERT DOUCH

THE Isle of Portland reaches out into the waters of the English Channel. Almost a solid block of stone, it is the most southerly point on the Dorset coast (1). Its greatest length from north to south is four miles and its maximum width one and a half miles, while its entire circumference is less than nine miles. The north of the island is low-lying, but half a mile inland the ground rises steeply to a maximum height of 496 feet above sea-level at the Verne. From here it slopes away gradually to the southern tip or Bill, 20 feet above sea-level. There has never been a town of Portland and the chief centres of population were originally eight hamlets. Today, three of these, Castletown, Fortuneswell and Chesil have merged to form the main settlement in the north or Underhill, as the district is called. On Tophill three more of the hamlets, Reforme, Easton and Wakeham have similarly run together. Weston and Southwell remain hamlets, while another settlement has grown up around the prison at 'The Grove'.

The 'Island' is, in fact, joined to the mainland by the Chesil Beach. But since this pebble bank extends westwards for ten miles before it meets the Dorset coast at Abbotsbury and could be used as a thoroughfare only with the greatest difficulty, the term 'island' is no real misnomer. Between Portland and the immediate mainland to the north runs the Fleet, a narrow arm of the sea, wide enough to make the approach across Smallmouth by ferry, before the building of the modern bridge, awkward and, at times, dangerous.

Until the middle of the 19th century the Portlanders seem to have had only limited contact with the mainland. Anyone not a native was a 'kimberlin' (2), and was treated with suspicion. Thus, it is not surprising that marriages were rarely contracted with outsiders. The solidarity of the island community is further emphasized by the fact that, as a rule, marriage did not take place until the woman was pregnant (3). If, after courtship, no conception followed, the couple separated, assuming that Providence looked unfavourably on the match. When pregnancy did occur, the two invariably became husband and wife (4).

¹ See fig. 1 Portland, co. Dorset, lat. 50° 33' N., long. 2° 26' W. The Ordnance Survey maps covering the Island are:—

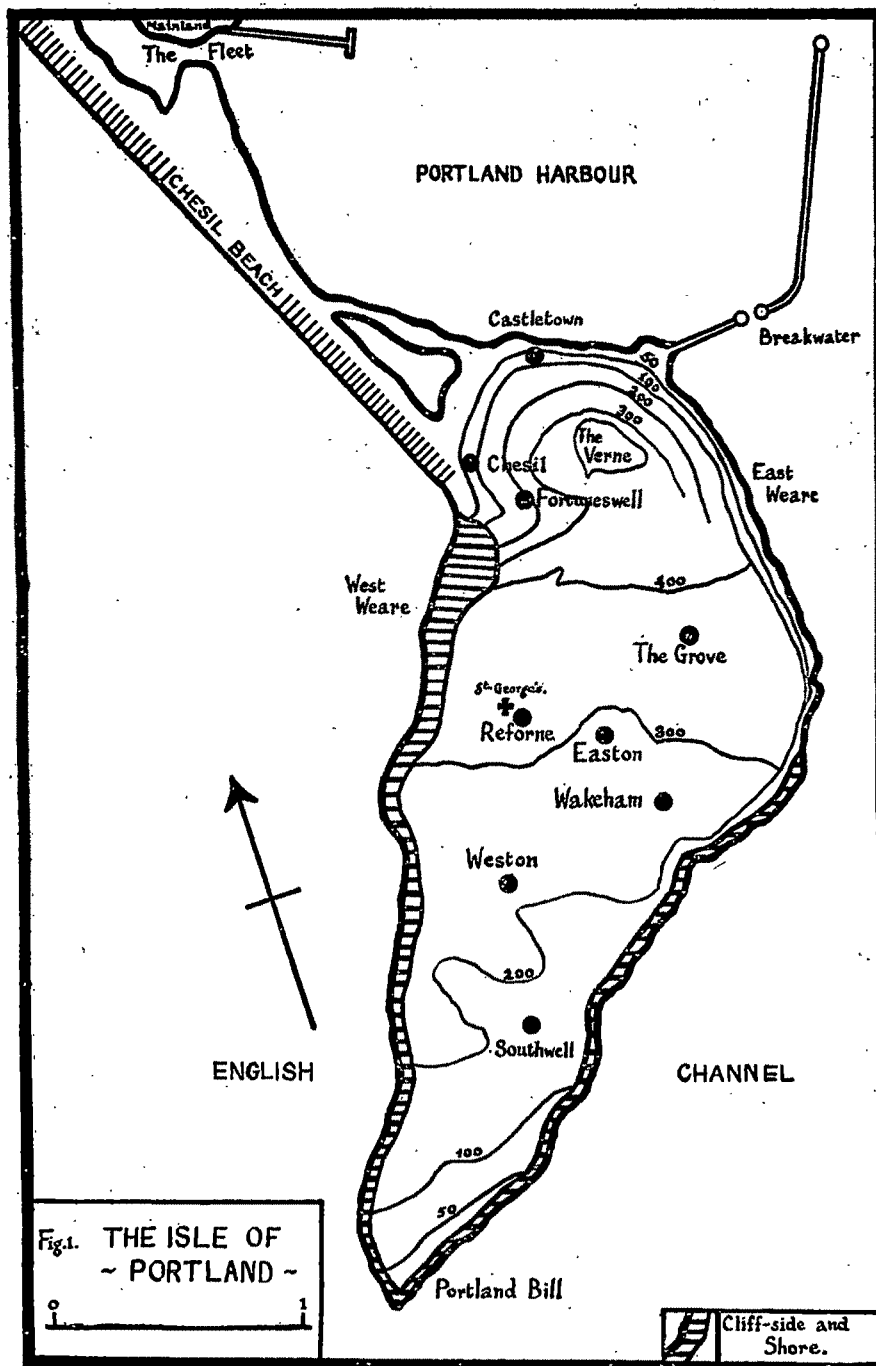
6-inch Sheets nos. 58 N.E., 58 S.E. and 60 N.E. (revised ed. of 1926 with additions of 1938).

25-inch Sheets, nos. 58—7, 11, 12, 15, 16 and 60—3, 7 (1929).

² Kimberlin or kimling, cf. *The English Dialect Dictionary*, ed. J. Wright, s.v. 'comeling'.

³ J. S. Udal, *Dorsetshire Folk-Lore* (Stephen Austin, Hertford, 1922), p. 199, suggests that this custom was not unknown in west Dorset and in Devonshire. It was perhaps connected with the old Danish marriage-contract known as 'handfesting': see J. Brand and H. Ellis, *Observations on the Popular Antiquities of Great Britain* (H. G. Bohn, London 1854), vol. II, p. 87. For a full description of the custom in Portland, see J. Hutchins, *The History and Antiquities of the County of Dorset* (J. B. Nichols and Sons, Westminster 1863, 3rd edition ed. W. Shipp and J. W. Hodson), vol. II, p. 809.

⁴ For the fate of some Westminster masons who broke this custom, see Hutchins, op. cit., pp. 820—1. They were stoned out of the island. Cf. p. 809: the birth of a deaf and dumb child to one of the unfortunate girls was held to signify God's wrath at the disregard of the custom.



ANTIQUITY

Constant intermarriage in such a small community necessarily limited the number of patronymics in use on the island. In all classes of records the Pearces, Combens, Lanos, Attwoolls, Stones, Whites and Flews predominate. Accordingly, it has been the custom for nicknames to be used to distinguish one person from another. So customary was the use of these nicknames that they are sometimes found even in official documents (5), and a man unknown to his neighbour by his baptismal name, might be easily recognized if called by his nickname. Today, these soubriquets are fast dying out, but an authentic list of more than five hundred of them which were in use early in the 20th century has been published (6). The derivation of some is easily perceived—'Long Robert', 'Fat Chap' and 'Shoemaker Ned': but whence came 'Tea-pot Spout', 'Lord Zinc' and 'Sunday Morning'?

While the stone trade, especially during Wren's rebuilding of London, must have done something to break down this insularity, the Portlanders remained a race apart. Even in the 20th century there were still some who had never been 'to England'. The experiences of those who did go to the mainland probably modified the island customs little. However he behaved outside the island, when in Portland a man did as Portlanders did. As long as there was no considerable immigration, the force of local custom further emphasized and perpetuated the results of physical isolation.

The peculiar customs and traditions which survived were by no means confined to family names and to marriage. As Thomas Hardy noted in the preface to his novel, *The Well-Beloved*: 'The peninsula . . . has been for centuries immemorial the home of a curious and well-nigh distinct people, cherishing strange beliefs and singular customs'. Today, most of these have died out: those remaining are obsolescent. Thus it has seemed desirable to collect all relevant information, both from written evidence and from old Portlanders themselves, before all traces have disappeared (7).

In a district where farming and quarrying have been the basic industries, there are many survivals connected with agricultural organization and practice. The most important of these is the existence to the present day of Open Arable Fields. These and their significance in proving that in one part of England, at least, the dividing balk between strips was not 'the invention of prejudiced ignorance' have already been noted (8). Since I hope to deal in detail elsewhere with the agrarian history of Portland, it will be sufficient to notice but briefly some of the peculiarities of the agricultural organization of the island. Thus, until the passing of the Tithes' Commutation Act in 1836, tithes were paid in kind (9), and as late as 1843 half the arable land was left fallow annually (10). The islanders also had their own units of measurement. A 'yard' equalled a quarter of an acre, that is a plot one pole wide and forty poles long. This measure was not

⁵ See Dorset County Museum, Dorchester, Box File 'Dorset 5', for a transcription of names and nicknames as registered in the official Tithe Map and Commutation.

⁶ C. J. Samways, *Portlanders' Titles: a Souvenir* (Portland, 1937).

⁷ I would like to thank all those who helped me by supplying information, especially Mr John Pearce and Mr Richard Lano of Portland, Mr J. W. Warren of Weymouth and Lt.-Col. C.D. Drew of Dorchester.

⁸ See Charles D. Drew: *Open Arable Fields at Portland and Elsewhere*: ANTIQUITY, vol. XXII (1948), pp. 79-81.

⁹ R. Pearce, *Methodism in Portland* (Charles H. Kelly, London, 1898), p. 122.

¹⁰ W. Stevenson, *General View of the Agriculture of Dorset* (Board of Agriculture Report, 1812), p. 201, and R. Pearce op. cit., p. 125. But cf. Hutchins op. cit. II, p. 809. 'The arable land is mostly common field, which is divided into three parts or divisions, one of which is every year a fallow'.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

unknown in other parts of England, but I have found no mention of an 'over-yard', equivalent to one and a half 'yards', outside Portland. The quarryman's foot contained twelve and a half inches, while the 'lug', used for measuring walls represented fifteen feet (11). These walls are another interesting survival. Built to enclose land, they sometimes replace the grass balks. But whereas the ownership of balks is laid down by custom and confirmed by law (12)—the southern lawnsed or balk belonging to the northern lawn or strip, and the eastern lawnsed to the western lawn, there is no custom defining the rights of ownership of walls. Each dispute over the latter has to be settled on its own merits.

The Court Leet, having survived the passing of Birkenhead's Law of Property Act (1925), still meets annually in Portland (13). The King is lord of the manor and his steward or Deputy calls the Court. Since there is no longer any communal cultivation in the island, the main business before the Court is not concerned with agriculture. Fines are still levied for encroachments on the waste and on the Commons, and when necessary, the Court negotiates for the sale of Common Lands (14), but practices connected with quarrying occupy most of the time.

Another important duty of the Court Leet is that of 'beating the bounds' of the manor. Surrounded as it is by water, Portland has the sea for its boundary except where it abuts on the neighbouring manor of Abbotsbury some four miles along the Chesil Beach. At this spot, a stone, 'Bound Stone', marks the division between the two. Thither, once every seven years, on Ascension Day, go the Court Leet officials to beat the bounds and, if necessary, to renew the stone. The Parish Accounts show that in the early 19th century cakes and beer were provided when the 'cullars' were carried to 'Bound Stone' (15) and ample refreshments are still a marked feature of the ceremony.

One of the most important manorial officials was the Reeve. In Portland the Reeve was appointed at the Michaelmas Court to serve for one year and was always that tenant paying the highest quit-rent who had not held office before (16). A woman could be Reeve. Mrs Rebecca Lowman, for example, held office in 1831, and three women, Margaret Heath, Margery Comben and Margery Spencer, were successively Reeves for the years 1845-7. But no one could be Reeve for a second term. Until 1833, the holder of the office was granted the use of a piece of land called the 'Reeve Plot' in return for his services (17), but after that date a money payment was substituted.

¹¹ Dorset County Museum, H. J. Moule's Notebooks, vol. VIII, pp. 173-4.

¹² See the judgment of Mr Justice Branson at the Dorchester Assizes, June 1935, in the case *Radford v. Mitchell and Peavitt*. The proceedings were fully reported in *The Dorset Daily Echo*, the decision being given in the issue dated 10 June 1935.

¹³ It previously met twice a year. For a full description of the Court, its officers and oaths, see: J. W. Warren, *The Island and Royal Manor of Portland* (Butler and Tanner Ltd., Frome and London, 1940), pp. 7-34.

¹⁴ In 1847 the tenants of the manor received £20,000 as compensation for the loss of their rights on the Great Common, when the Breakwater and Prison were built. Fines are also paid for the erection of telegraph poles on roadside waste.

¹⁵ See *The Parish Book of Portland*, 1779-1820, p. 89 et passim. Mr J. W. Warren kindly lent me this book.

¹⁶ See Public Record Office: Augmentation Office, Parliamentary Surveys E.317/12, which lists the tenants and their rents and states the customs of the manor in 1650, cf. P.R.O.: L.R. Misc. Books, no. 214, pp. 93-150 for a rent roll and less detailed statement of customs for 1608.

¹⁷ For its location and area see the Tithe Map and Apportionment Award, Portland, Dorset 10/173, parcel no. 306. These are held by the Tithe Redemption Commissioners, Finsbury Square, London.

ANTIQUITY

It was one of the chief duties of the Portland Reeve to act as collector of the rents. The quit-rent paid to the King for the whole of the island amounted to £14 14s 3d. Threepence an acre was paid for land, a farthing for the site of a cottage and a halfpenny for a cottage with a garden. The rents of all tenants were entered by the Reeve on a Reeve-Pole or Reeve-Staff (18). This Reeve-Staff was a pole of mahogany, deal, or pine, from one to one and a quarter inches square and varying in length from seven to twelve feet. On it were symbols representing the five tithings and the amount of rent to be paid by each tenant. A 'V' represented Chesil, a 'W' Easton, a hollow circle Southwell, a circle with a cross in it Wakeham, and a cross between parallel lines, Weston (FIG. 2). Notches and scratches signified the rent owed. Thus a whole notch right across the face of the pole equalled a shilling and half a notch sixpence. Similarly a full scratch represented a penny, half a scratch a halfpenny and a quarter of a scratch a farthing. Dots separated one amount from the next (FIG. 3). In order to shorten the

V	CHESIL	○	SOUTHWELL
W	EASTON	⊗	WAKEHAM
⊠	OR	⊠	WESTON

Fig. 2.
Diagram of Tithing-Marks on Reeve-Staff.

pole, a new method of cutting was introduced as late as 1885. Under the new system, a diagonal notch, which had not been used before, equalled a pound. A straight notch still signified a shilling and half a notch sixpence, but now there were also three-quarter and quarter notches representing ninepence and threepence. Single pence and fractions of a penny were shown as before. The Reeve Staff had to be produced at Court Leet as evidence of collection and payment: it was then returned to the Reeve. Many old staffs are still in existence and are highly prized by the descendants of the original holders. There are four in the Portland Museum, and others in the County Museum at Dorchester (19).

Portland not only retains traces of the medieval system of cultivating the land: it was also unique in its methods of land-transference. A 19th century custumal states:

¹⁸ 'The tenants hold their lands . . . by custom of a Staff, and not by Copy, the rents being set on the said Staff, and every year the Reeve for the time being is to increase or decrease every Tenant's Rent, by making a new Staff . . .', P.R.O., E. 317/12.

¹⁹ For a full description see F. W. Galpin, *The Portland Reeve Staff. The Reliquary and Illustrated Archaeologist*, April, 1903, pp. 73-80 and 216.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

'We also present that we have three ways to dispose of our lands, namely in free church gift, Surrender at Court, and last will and testament' (20). The last method needs no explanation.

When land was conveyed by Surrender at Court the two parties attended the Court Leet, where the holder surrendered the property in question to the lord. Then, when the price had been fixed and a fine, called a 'Landchief' and equal to the quit-rent paid on the property, had been handed over, the buyer became the new tenant. Thus, for example, on 25 October 1731, 'To this Court came John Peters and surrendered into the hands of the Lord King . . . one close of pasture called Colecroft . . . with all its rights . . . for the use of Andrew Stone, his heirs and assigns forever. Whence there fell to the Lord King as a Landchief twopence farthing. And . . . the Lord, through his Seneschal, gave him seisin by the staff'. The rent to be paid was twopence farthing and the purchase price was £40 (21). If the heir of the seller wished to redeem land thus sold, he could do so by paying the purchase price to the buyer before two or more tenants of the manor within a year and a day of the original transaction (22). Surrender at Court was still used as a means of conveyancing in the 19th century. In

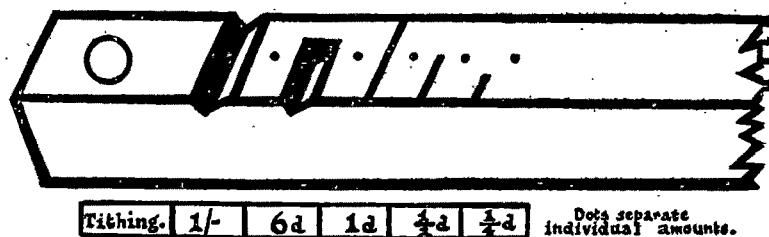


Fig.3.—Diagram of Reeve Staff.

1821, when the lands of Mr John Gilbert were sold, it was declared 'That the purchase deeds of the several lots shall be prepared . . . at the expense of the respective purchasers, unless they shall choose to take the same by Surrender in Court, at their own expense (23)'.

In the Middle Ages the Parish Church was often used for business transactions (24), and the third way of transferring land in Portland was a survival of this practice. Transference of property by Church Gift continued until 1925 (25). By this method

²⁰ See *The Historic Court of* 1846, printed in Warren, op. cit., p. 19. The two 17th century surveys also describe the custom.

²¹ This example, a translation, is taken at random from the Court Rolls which are almost entirely composed of these Surrenders. The entries are in Latin until 25 October 1732, and thereafter in English. The rolls cover the years 1670-1773: P.R.O., L.R. 3/17/1-4.

²² P.R.O., E. 317/12.

²³ This sale was one of the largest the island has known. Mr J. W. Warren kindly lent me his copy of the original Bill of Sale.

²⁴ See S. O. Addy, *Church and Manor: a study in English economic history* (Geo. Allen and Co. London, 1913). There is a small room on the south side of the parish church of Breamore, co. Hants, with an arch bearing an inscription in Anglo-Saxon: 'Here the agreement is made'.

²⁵ The last entry in the Register of Church Gifts in the Parish Chest of St. John's, Portland, is dated 30 July 1925. Unfortunately the record does not begin until 1890.

ANTIQUITY

the parties concerned met in the parish church and with at least two tenants of the manor as witnesses transferred the property from one to the other. Originally the conveyance was purely verbal. The extent of the land was declared, the parties joined hands, the priest laid his hand on theirs and the transaction was complete (26). In the 17th century (27) a father who wished to leave land to his daughters stood in the church porch after evensong and in the presence of the congregation named his daughters in turn, specifying the exact boundaries of the lands which each was to inherit. After this the congregation would rise up and bless the daughters by name. Although the term 'Church Gift' is not mentioned, it seems clear that this is the custom described. By this time the presence of the priest was no longer necessary. At a later date the agreement was often committed to paper, but it was not until 1845 that the law demanded that a definite deed be made. After 1845 it was usually the practice for the Church Gift to be drawn up, sometimes by the schoolmaster, taken to the Church, and signed in the presence of two witnesses (28). The form of the document varied little: 'Memorandum That on the . . . day of . . . in the year . . . I . . . of Portland, in the county of Dorset, came into the parish church dedicated to St. George, and did then and there, according to an ancient custom of the island, time out of mind, in the presence of the undersigned witnesses freely give unto . . . And this I acknowledge to be my free Church Gift. In witness whereof I have set my hand and seal this said day and year above written' (29). A register, in which these transactions were to be entered, was kept in the church, but it was not always used since many people wished to keep their business private. Old Portlanders still strongly resent the abolition of this cheap method of land transference by the Law of Property Act of 1925.

In conjunction with these customs of land transference, it is interesting to note that long before the passing of the Married Women's Property Acts the wife of a Portlander could inherit property in her own right and bequeath it by will independently of her husband (30). Moreover, by marrying a Portland girl who was a tenant of the manor, a man became a freeman of the island with the right to work in the quarries.

A peculiar method of distributing property as well as of conveyancing existed in Portland. It has been suggested that the tenure of gavelkind obtained in the island (31). This suggestion seems to be incorrect. The custom of gavelkind strictly refers to tenure only, that of holding land in return for the payment of food rents. Certain customs often accompanied this tenure. Thus land descended upon intestacy to all sons equally, a widower was entitled to half his wife's land so long as he remained unmarried, a widow received half of her husband's land as a dower, a felon's heirs could inherit his father's lands, an infant could alienate his land when he reached the age of fifteen, and the land was devisable (32).

²⁶ For some account of this custom, 'marrying the land' as it was called, see *Somerset and Dorset Notes and Queries*, no. 4, p. 62.

²⁷ See British Museum, Stowe MSS. 597, fol. 42b et seq.

²⁸ See *Notes and Queries*, 9th series, vol. VIII, p. 81.

²⁹ See e.g. Dorset County Museum, Church Gifts, dated 1770, 1842 and 1861: nos. 11,061, 11,060, and 11,058.

³⁰ See Hutchins, op. cit., vol. II, p. 809.

³¹ See e.g. Hutchins, op. cit., vol. II, p. 811, quoting J. Smeaton, *History of the Eddystone Lighthouse*. (2nd edn., London, 1793).

³² T. Robinson: *The Common Law of Kent or the Customs of Gavelkind* (3rd edn., London 1822), Part II.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

In Portland partible inheritance was the custom in cases of intestacy. But the division of land amongst all sons equally when a person died without leaving a will, was only one of the incidents of gavelkind. Thus the custom of partible inheritance did not mean that the custom of gavelkind prevailed in that district. Furthermore, a widow in Portland received only a third of her husband's land, whereas in Kent she would have received a half. At the most we can loosely say, in the words of the 17th century survey, that in Portland land was 'descendible in gavelkind'. When a man or woman died intestate, the sons shared the land equally: daughters could not share in the inheritance unless there were no sons (33). Whether gavelkind was ever the custom in Portland as it was in Kent, I have found no evidence to decide. It is perhaps significant for the medieval organization of the manor that the Domesday Book entry for Portland lists five slaves, only one villein and ninety bordars.

Although land which was partible might be split up amongst its owners, it seems that the economic advantages of maintaining the property undivided were early realized (34). In Portland, actual division would have led to chaos. Individual strips were already small and further sub-division would have resulted in the creation of minute parcels of land useless as economic units. Property was therefore held in undivided shares. In 1821, John Gilbert offered for sale amongst other properties, 'five undivided ninths of a piece of arable land in the South Field by estimation 1 rood 13 perches (35)', and an entry in the Church Gift Register refers to the transfer of 'one seventh of a fifth part of a land in Yeolands' (36). Although partible inheritance was ended by the Law of Property Act (1925), many Portlanders still own undivided shares. These divisions do not affect cultivation, but they are of real importance. If land is sold all owners have to be traced. Before undivided land can be quarried the consent of every owner has to be obtained, though if land once opened is left for a while, only the permission of the largest part-owner is required to recommence working (37). Furthermore, if stone is quarried from an undivided strip, duty has to be paid to the several owners. Similarly the road dues (38) have to be shared.

Quarrying, like agriculture, furnishes many examples of curious customs and survivals. This industry is now of national importance, and its history and organization, which demand special attention, can only be briefly noticed here (39). While Portland quarries were worked for local purposes and for limited export in the Middle Ages, no large scale quarrying took place until the 17th century. Difficulties of digging the stone, which often lay thirty feet below the surface, poor transport facilities and the lack of any real demand prevented their development (40). Building in London, under the direction of Inigo Jones and more especially of Sir Christopher Wren, laid the real foundations of the industry.

³³ P.R.D., E. 317/12.

³⁴ See E. A. Jolliffe, *Pre-Feudal England: the Jutes* (Univ. Press, Oxford 1933), p. 20.

³⁵ Bill of Sale, see n. 24.

³⁶ Register of Church Gifts, no. 121.

³⁷ Ex. Inf. Mr J. Pearce.

³⁸ See below.

³⁹ See *Victoria County History of Dorset* (Constable, London, 1908), vol. II, pp. 338-43. I hope to deal in detail with this subject later.

⁴⁰ See D. Knoop and G. P. Jones, *The English Medieval Quarry*, *Economic History Review*, vol. IX (1937-9), pp. 22-4.

ANTIQUITY

Most of the early quarries were on Common or on Crown Land, and only tenants of the manor could open and work them. In 1664, the islanders petitioned the king for a duty of twelpence per ton to be levied on all stone exported from the island (41). This duty which was already imposed by ancient custom (42) was to be shared equally between the king and his tenants. Charles II was more generous than the islanders had asked. In return for the damage done to the Commons by quarrying and for the loyalty which Portland had shown during the Civil War, Charles, in 1665, granted ninepence of the duty on every ton of stone from the Commons to the islanders, and kept only threepence for himself (43). For every ton of stone raised in the demesne lands 'time out of mind' threepence was paid to the inhabitants until Queen Victoria increased the amount to sixpence. If the demesne lands were leased, the farmer received sixpence of the Crown's share. Stone dug from the Commons by the tenants for their use within the manor or from the demesne by the lord for his own use paid no duty. Similarly tenants could raise what stone they wished in their own lands free of charge (44).

Besides this duty of a shilling a ton, there were other dues to be paid. On the east and west side of the island are the 'Weirs' (45). Below the cliff top and stretching down towards the sea, the weirs were formerly the dumping ground for the rubble which has to be cleared before the marketable Portland stone is exposed. Rubble from quarries on the Commons could be deposited free of charge, but stone merchants had to pay sixpence on every ton of wrought stone for rubble which was dumped from private grounds (44). The East and West Weirs were divided into smaller weirs: thus, for example, in the Parish Book, Green, Knight's, Church Point, Hind's and Gregory's Weirs are noted. These weirs were often rented for their profits, and like land, were frequently held in undivided shares. Thus in 1911 fourteen people had an interest in Gregory's Weir: the Bath and Portland Stone Firms Ltd., holding 7/12 and 1/200 were the largest shareholders and Mr W. Comben (Prospect Place) with 1/1200 was the smallest (46). In 1937 the last payments for depositing rubble were made. Quarrying activities have moved inland and the overburden is no longer sent to the weirs. When quarrying near the cliff-edge is resumed, rubble dues will undoubtedly be paid again.

Rubble dues are temporarily obsolete, but twopence per ton still has to be paid to the landholders over whose property stone passes on its way from the quarries to the road. Early in January each year, the quarry owners furnish the assessor with details of the amount of stone dug in their quarries during the past twelve months (47). With the help of a map, the latter decides who are entitled to receive these road-dues, and on the 30 January the money is shared out. In 1930 the record amount of £617 16s 1d was

⁴¹ *State Papers Domestic*, 1663-4, p. 618.

⁴² See Answer to Article of Enquiry no. 10, 1 June 1678: L.R. 3/17/1.

⁴³ *S.P.D.*, 1665-6, p. 41. This royal grant has to be renewed at the accession of each king. The extra threepence on every ton granted in 1665 is paid into 'His Majesty's Grant Fund'. This money, vested in trustees, is used to help finance schemes for the general welfare of the island. Two poor houses and St. George's Church received grants from this fund.

⁴⁴ See *The Historic Court of 1846*, printed in Warren, op. cit., p. 25.

⁴⁵ Weir, wear, weare, ware: Hutchins, op. cit., p. 822, note a, suggests that the word is of Celtic derivation and means 'rough, wild ground'.

⁴⁶ The late Mr J. Waight of Portland kindly supplied these figures. I have not been able to discover how such minute sub-divisions came into being.

⁴⁷ It is not known when this custom originated. Mr J. Pearce has for many years now allocated the road dues and holds the records of the payments made since the middle of the 19th century.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

divided in this way : in 1947, as the industry began to recover from the effects of the war, the sum to be allocated was £200 2s 2d. This division is no simple task. There is only one twopence for each ton : it is twopence to be shared amongst all the landowners, not twopence for each of them. Moreover the number of strips over which the stone has passed may be considerable, and there is always the added complication of undivided shares.

There is another important island custom concerning roads or 'ways', as they are called. If a public road is destroyed by quarrying operations, it is the custom for the proprietor of the adjoining property to allow a way through his land. In return for this concession he receives the tonnage dues for all stone raised under the old road (48).

The introduction of modern machinery has done much to alter methods of work in the quarries. But until the middle of the 19th century they had probably changed little since the days of Wren (49). Manual labour still did most of the work, horses were still the main form of transport (50), and export was still mostly by sea. The quarryman still received only a small weekly allowance and a full scale settlement only took place quarterly (51). But the mechanisation of the last hundred years has wrought great changes : although old tools, such as the 'kevil', half hammer and half-pick-axe, are still used, the mechanical excavator, the electric drill and crane, the lorry and the railway have revolutionized the old organization.

Any social customs which characterized quarry life have likewise disappeared. Of them, no record is left. There is one exception : we have full details of the celebrations which attended a quarryman's marriage (52).

Before the changes of the late 19th century, quarrymen usually worked in gangs of four men and a boy in each quarry. On the morning of his wedding, the bridegroom would send a cake, a bottle of whisky, a bottle of port-wine, a bottle of peppermint and about a gallon of ale to his workmates. When this arrived in the quarry, a flag was hoisted on the mast of the crane as a sign that all was well and inviting those working nearby to join the celebrations. Festivities continued all day, a favourite pastime being 'Rattlehat'. In this game, five pennies were placed in a hat, and the gang which threw most 'heads' was the winner. Fresh supplies of liquor were regularly obtained. Before the bridegroom started work again he visited his quarry and 'jumped'. This ceremony was usually conducted by the oldest man amongst those celebrating the marriage. He appointed two tellers, whose duty it was to take the name of anyone who failed to observe the rites. Then, all hats having been removed, preparations were made for the 'jump'. The man last married held one end of the 'jumper' (an iron rod used in the quarry for

⁴⁸ *The Historic Court of 1846* : see J. W. Warren, op. cit., p. 26.

⁴⁹ See R. Darnley, *The Geology of Weymouth, Portland and the Coast of Dorset* (New and enlarged edition. Weymouth and London, 1884), p. 94.

⁵⁰ An important exception to this was The Merchants' Railway. Opened in 1826, it was the first railway in Dorset. It was used to convey stone from Top-hill to Castletown Quay. It worked, with drums and cables, by gravitation, the loaded trucks going down causing empty ones to ascend on another set of rails.

⁵¹ Dorset County Museum, H. J. Moule's Notebook, vol. ix, pp. 146-9, shows that the quarterly settlement was still the custom in 1896. In the 18th century it was a half-yearly settlement : see E. L. Elanchard, *The Watering Places of England : Weymouth* (W. J. Adams, London, 1850), pp. 46-7.

⁵² I am indebted to Mr B. R. C. Smith of Southwell, Portland, who kindly gave me a description of the ceremony as he witnessed it when a boy.

ANTIQUITY

drilling holes) and the man who was supposed to be married next held the other end. All the married men lined up behind the former and all the single men behind the latter. Fines were imposed by the tellers for being out of place, for wearing a hat and for talking. Then the M.C. recited the service :

‘ Young men and bachelors, I bid you all adieu,
Old men and married men, I’m coming on to you ’.

Over the rod went the victim, helped by two of his workmates with sticks or shovels. Then he jumped back again to the words,

‘ Old men and married men, I bid you all adieu,
Young men and bachelors, I’m coming back to you ’.

Finally he jumped for a third time, as the first verse was recited again. Thus the ceremony ended and the newly-wed joined the ranks of the married men. Fines were counted and the celebrations continued.

Besides inheriting strange customs in the organization of their everyday affairs, old Portlanders clung firmly to numerous traditions, beliefs and superstitions. Tradition said that the island had been peopled by a three-fold settlement: ‘ first came the Combens: then from Ireland, by way of Cornwall, the Pearces, and thirdly the Whites who came from the sea, Dover way (53) ’. According to old Portlanders there had never been any peasants on the island (54), and at the other end of the social scale no one could be a gentleman if he or his ancestors to the third generation had been traders or had owned less than five hundred acres of land (55). It was also believed that ‘ Bow and Arrow ’ or ‘ Rufus ’ Castle, now situated on the very edge of the cliff on the south-east of the island, was once in the middle of the isle, and that there was formerly a butcher’s shop on the now dangerous stretch of water off the coast, the Shambles.

A firm belief in the supernatural accompanied the acceptance of these traditions: animals and men were endowed with more than earthly power. Today many remember men and women who could bewitch their fellows. There was, for example, ‘ Wold Maremaid ’, the keeper of ‘ The Mermaid Inn ’, whose very presence foretold impending disaster (56). Similarly women were not welcome when fishing was in progress since their presence was held to have a bad influence on the catch. The sight of a goat or a linnet was an evil omen, and it is still bad luck to call a rabbit by its proper name—‘ those furry things ’, ‘ Wilfreds ’, ‘ bunnies ’, or ‘ those long-eared things ’, but never ‘ rabbits ’. Even a tree possessed supernatural power. On Midsummer Eve 1823, a mother brought her young child from Weymouth with the intention of passing it through a cleft ash tree in the grounds of Pennsylvania Castle at sunrise to cure it of hernia (57).

Traditional ceremonies, most of which had died out by the middle of the 19th century, existed side by side with tradition and superstition. A Portland wedding was a colourful affair. After the ceremony the bridal couple walked round the island followed by their guests in pairs. Calls were made at friends’ houses and festivities were kept

⁵³ C. King Warry, *Old Portland Traditions* (Warren & Co., Portland c. 1914), p. 29. Documentary evidence suggests that this tradition is incorrect: Pearce is the most ancient island name which survives today.

⁵⁴ Mrs King Warry, *The Status of Peasantry in Portland: Proceedings of the Dorset Natural History and Antiquarian Field Club*, vol. xxx, 1908-9 (Dorchester 1909), p. 73.

⁵⁵ C. King Warry, *op. cit.*, p. 52.

⁵⁶ W. Sherren, *The Wessex of Romance* (Chapman and Hall, 1902), p. 90.

⁵⁷ Mrs King Warry, *The High Place* (typescript copy in Dorset County Museum Library), pp. 18-19.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

up for two or three days. One bride was followed by a hundred and ten guests, besides numerous spectators on her wedding day (58). St. George's Church had no peal of bells, so anvils, pistols and guns provided the musical accompaniment for the procession. An island funeral likewise witnessed a procession of couples. On this solemn occasion those attending were dressed in sad garments, and if the dead person were a child, the bearers would be young girls clad in white (59). A Portland funeral in the middle of the last century seems also to have been unique in that a funeral sermon was preached (60).

Besides these occasional ceremonies, there were also those which took place at fixed festivals in the year. Until late in the 19th century, the Wednesday of the seventh week after Christmas was 'Binding Day' (61). On that day, men and women took anything which they could from their neighbours, without intervention by the law, the trophies being redeemable on the payment of a small sum. May Day saw the cows turned into the Commons, processions in the streets with the girls dressed in white, dancing and general jollification. There was also the old custom of keeping the household fires alight, from November to May, and then forbidding them to be lit again (except for essential cooking purposes), from May to November (62). In the 18th century at Hocktide two Underhill women held a rope stretched across the road near Verne Yeats and every man who wished to ascend to Top-hill had to forfeit a kiss before he could pass (63). And on 5 November Portland Fair was held, but no records describing it seem to have survived.

Few of these traditions and customs exist today. Their survival depended on the isolation of Portland and on the absence of any large influx of population. As Hutchin's story of the Westminster masons shows, the force of local custom was strong enough to permit the absorption of a small number of immigrants or to enforce their expulsion if they broke the customs of the island. But as soon as the island came into close touch with the outside world it was inevitable that these customs would soon disappear. The middle of the 19th century marked the turning point. The building of the prison and the Breakwater and the construction of military fortifications on the Verne led to a great influx of population. Whereas the population in 1801 was under 2,000, it had arisen above 15,000 by 1901 (64). At the same time improved communications with the mainland (65), and better transport facilities generally meant that more people went to and from the island. Portland's isolation was at an end, and the old customs were doomed.

In 1949 the Court Leet, the tonnage and road dues on stone and the skeleton of the Open Fields still remain. The bounds are beaten, a few nicknames are heard, and a symbolic Reeve-Pole is still cut. Some of the old Portlanders believe in the

⁵⁸ Fido Lunettes, *An Historical and Descriptive Account of the Peninsula of Portland* (S. McDowell, London, 1825).

⁵⁹ Mrs King Warry in T. Perkins and H. Pentin, *Memorials of Old Dorset* (Bemrose, London, 1907), p. 186.

⁶⁰ See J. Brand and H. Ellis, *op. cit.*, vol. II, p. 279.

⁶¹ See A. M. Wallis, *The Portland Stone Quarries: Proc. Dorset Nat. Hist. and Antiq. Field Club.*, vol. XII, 1890-1 (Dorchester 1891), p. 194. The origin and significance of this custom, like those of many which follow, are obscure.

⁶² An old man who died c. 1830 was the last to adhere rigidly to this custom: see Mrs King Warry in Perkins and Pentin, *op. cit.*, p. 186.

⁶³ Mrs King Warry: *The High Place*, p. 44.

⁶⁴ Census figures: 1801, 1,619. 1901, 15,199.

⁶⁵ In 1839 a bridge replaced the old ferry-boat on ropes.

ANTIQUITY

supernatural and rabbits are still evil omens. But on the whole the old order has changed, as life in Portland has joined the main stream of development.

Isolated as Portland has been, the history of this particular island throws some light on wider historical issues. The existence of the grass balk between strips in the Open Fields is established beyond doubt for this English manor, and perhaps for all manors on the light, dry soils of the limestone and chalk country. The transformation of the balk into a linchet where the soil is turned down-hill is also illustrated, and the theory that some linchets, at least, are intimately connected with Open Field agriculture is thus proved (66). The Court Leet exemplifies in detail the working of this formerly widespread organ of local government. The Reeve-Staff suggests a primitive method of reckoning to help those who could not read or write and is reminiscent of the Exchequer tallies. Whether the custom of partible inheritance is in any way connected with a possible Jutish settlement west of the Hampshire Avon, I have no evidence to decide (67).

But it is the difference between Portland and the mainland which gives the study of the Island's customs and traditions its real interest and value: they represent the way of life of a unique community settlement. Here, on an island, separated by only a narrow stretch of water from the rest of Dorset, a distinct people with a distinct set of customs survived within living memory. In the words of Peter Mundy who visited Portland in 1635, 'A strange alteration between this and the Maine, the distance being soe small' (68).

⁶⁶ See Drew op. cit., pp. 80-1.

⁶⁷ See Drew op. cit., p. 80, n. 7. The entire question of the origins of the peculiar Portland customs I am unqualified to discuss. The hamlet settlements and the custom of partible inheritance perhaps suggest Celtic influence.

⁶⁸ N. M. Richardson, *The Travels of Peter Mundy in Dorset, 1635*, *Proc. the Dorset Nat. Hist. and Antiq. Field Club*, vol. XLII, 1920-1 (Dorchester 1922), pp. 48-9.

Canterbury Excavations, 1944-8

by SHEPPARD FRERE

ON 1 June 1942, occurred the German air-raid which destroyed about one-fifth of the old part of the City of Canterbury. It was soon realized that a unique opportunity existed of discovering something of the archaeological past of the city before rebuilding again concealed the wide areas now exposed.

It was not only an opportunity but a duty; for modern rebuilding, with its deep foundations, would be bound to destroy a very large part of whatever evidence had survived until the present. The Canterbury Excavations Committee was accordingly formed by the initiative of the local Archaeological Society, and excavations under a supervisor lent by the Ministry of Works began in August 1944, and have continued three times a year up to the present.

Canterbury is one of the few Romano-British towns where there are good grounds for supposing a continuity of occupation through the Dark Ages (1). Thus though there was little to hint at any pre-Roman occupation, from the Roman, Saxon, and Medieval periods much was to be expected. Yet such a prediction has not turned out wholly true. On the one hand evidence for a pre-Roman Belgic settlement is accumulating; but little of Saxon date has been found, and the circumstances of the excavations have limited the extent of the medieval discoveries.

The town could not be investigated purely as an archaeological problem, digging on sites which seemed promising and following out the plan as on an open site like Silchester or Verulam. There was a double call. Rebuilding was ever imminent, and the first sites to be rebuilt were likely to be along the main street-frontages. Attention was necessarily devoted first to them.

The work of Mr W. Urry on the Cathedral Rentals has shown that much of the existing town plan, and many even of the existing property boundaries along the principal streets, go back at least as far as the 12th century. Almost all the main streets, Burgate Street, S. George's Street, S. Margaret's Street, and Watling Street, have cellars along their entire frontages. And so these cellars, mainly excavated in the 16th-18th centuries, have removed the medieval layers inside the houses, and often even the medieval walls themselves have been replaced. In the event, therefore, the medieval remains have been mainly confined to rubbish pits of various purposes, the contents of which are limited to broken pottery and bones. Few traces of medieval building have been found.

It is consequently with remains of the Roman period that the excavations have principally been concerned; and this is as it should be, seeing that the Roman period constitutes one-fifth of the history of the city, and about it least is known. The medieval history of Canterbury is well documented, and it may be that the most valuable result of the Excavations as regards medieval studies will be in the production of a dated series of pottery types.

Medieval pottery is very regional in its designs, as is now well-known; and it will be useful to have a large Kentish series. But even here there are difficulties, for it is but rarely that the pits produce any object of external dating value, and the dating of coarse pottery is still too largely subjective.

¹ See Collingwood and Myres, *Roman Britain and the English Settlements*, 428 and *passim*.

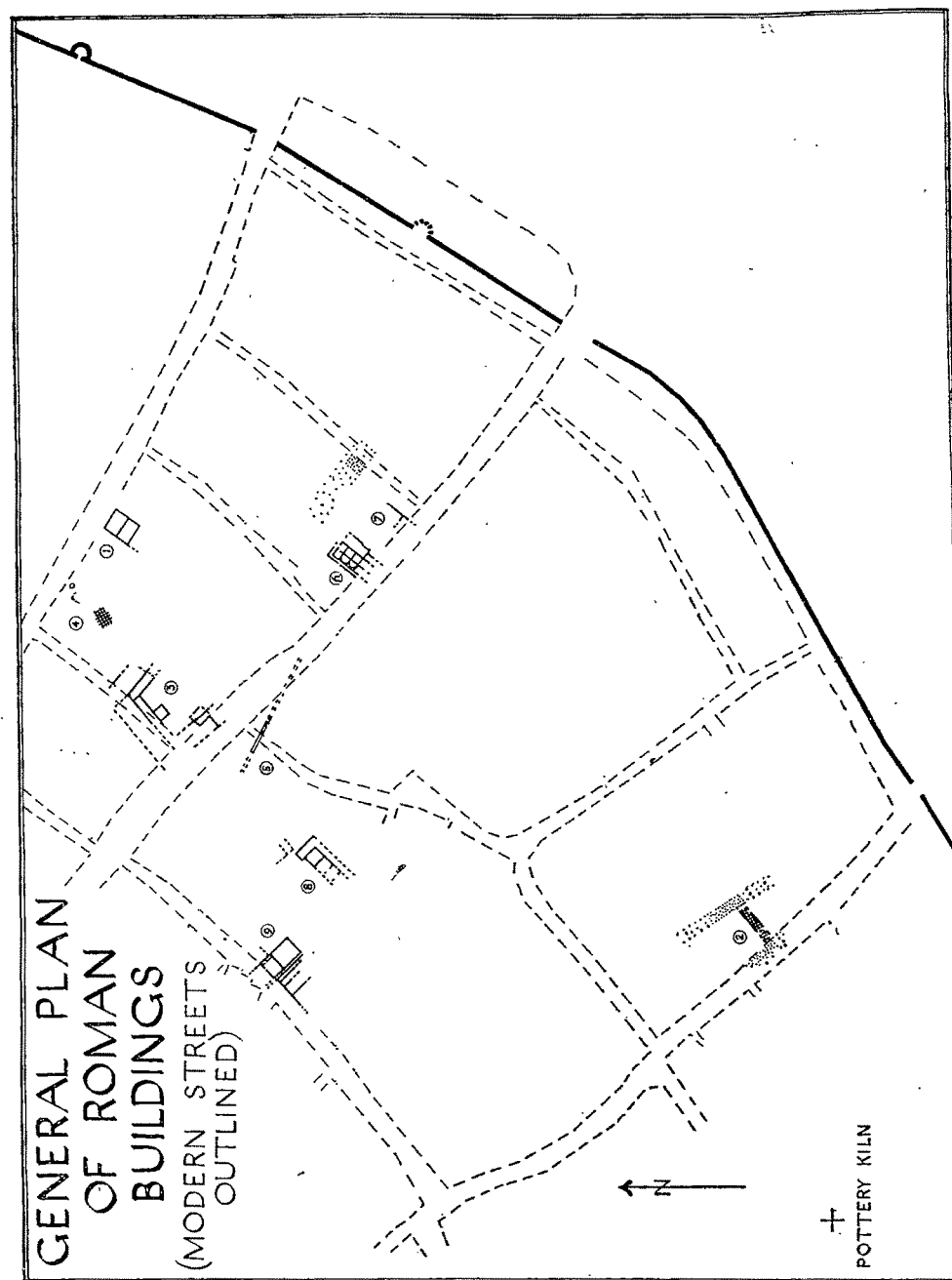


FIG. 1. ROMAN BUILDINGS IN CANTERBURY

CANTERBURY EXCAVATIONS, 1944-8

That Canterbury was the Roman city of Durovernum Cantiacorum has long been established; but having been in continuous occupation ever since, little was known either of its history or town plan: that little has recently been summarized (2). Our principal source of information was the account published by James Pillbrow in *Archaeologia* XLIII of his observations when trenching the streets in 1867 for main drainage. He was in advance of his age, by British standards, to have noted anything at all; but it cannot be said that his article leaves us very much wiser, partly owing to the small scale of his plan, and partly owing to his failure to notice the Roman streets as distinct from the house-walls. But at least we can be certain of two things; first that some of the chief buildings lay under the High Street near the County Hotel west of the area now laid bare, and secondly that wherever the Roman streets were, they do not underlie their modern successors.

It must be confessed that the Roman street-plan still eludes us. This is due partly to the fact mentioned earlier that we have been digging systematically in cellars, not laying down long trenches as over an open site, and partly to the prevalence of medieval rubbish pits in the cellars. The problem of soil-disposal in a small cellar is always acute, and a four-foot trench might easily cross the line of a Roman street, yet be wholly occupied by a series of deep intersecting pits of later date. Only when the time comes to trench across the wide spaces behind the cellars may we hope to learn something coherent of the streets.

And here another factor, the great depth of deposits, must be considered. The height of 'natural'—that is undisturbed—soil about sea-level varies considerably from site to site. In Burgate Street the cellars are about 7 feet below street level, and natural soil a further 8 feet lower, at 30 feet O.D. At the north end of the Butchery Lane cellar it was even lower, at 24 feet O.D. But it rises towards the South, so that in parts of S. George's Street it is not more than 2 feet below cellar floors (35 feet O.D.), unless a pit is encountered.

Hitherto the work has almost all been done by volunteer helpers, schoolboys and schoolgirls, university students, and others on holiday. Paid help has rarely extended beyond breaking some of the concrete cellar floors. The reason has been that large-scale rebuilding has been continually put off, and it was felt that as long as we could carry on in this way, other cities—Dover, London, Southampton, Exeter—with more urgent claims should have the field clear for appeals.

But this scheme of things has prevented much digging from the surface, except when a Roman building was known to exist and a surface-trench was necessary to complete the plan. Few volunteers, and those only the most muscular, can be expected to dig regularly at ten feet down. It is evident that money on some scale will have to be expended—and first collected—if we are to cut hundred-yard trenches from street to street.

Some help can be got by the mechanical excavation of the upper levels once these are known to be of little value; but there is always the danger of thus destroying important Saxon or medieval remains unless soundings are taken first. A start, however, was made in 1948 in the yard of the Rose Hotel, west of Rose Lane, where an earlier trench had uncovered part of a 2nd century house covered by six feet of 18th century earth; here a wide area was lowered five feet by a grab, and parts of two other buildings discovered; though once again the problem of soil disposal became acute.

The time has now come, however, for work on a larger scale. The rebuilding of big areas of derelict ground is in sight, and the Excavation Committee has issued an appeal for £5,000 in order that paid labour and proper equipment may be employed.

² See *Roman Canterbury* No. 1, *The City of Durovernum*, by Sheppard Frere, published for the Excavation Committee by the Medici Society.

ANTIQUITY

THE BUILDINGS

The town, about 130 acres in extent, seems to have been of the garden-city type, resembling Silchester with its scattered buildings more than the crowded plan of Caerwent. At the time of the Conquest, Belgic occupation seems to have been concentrated on the crest of a slight rise of the underlying loam between S. George's Street and Watling Street (as far as is known at present).

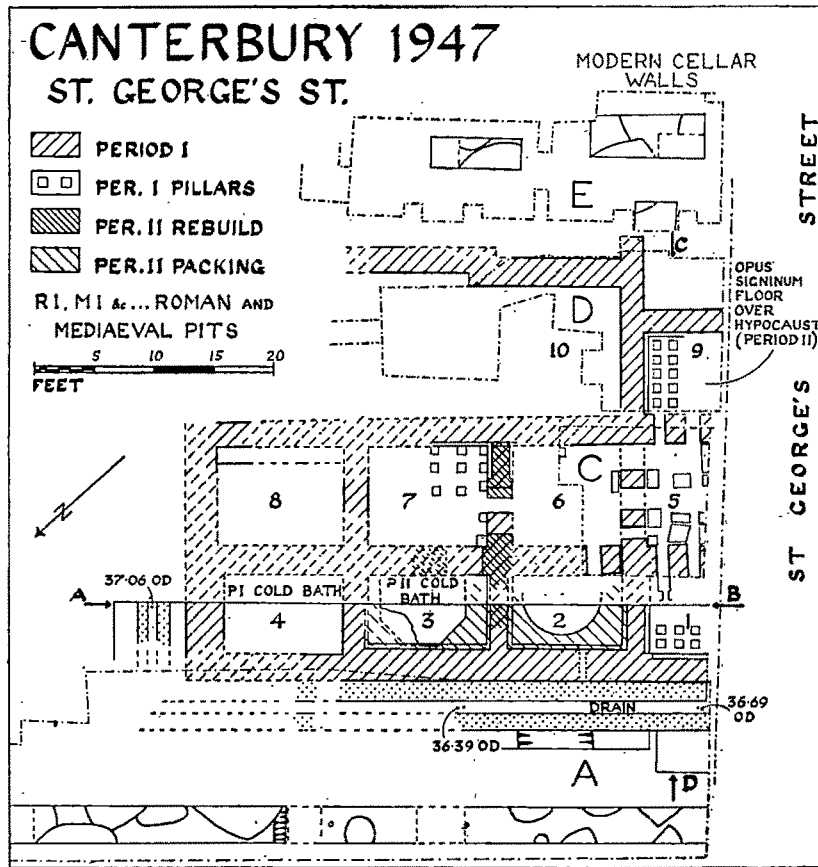


FIG. 2. PLAN OF BATH HOUSE IN ST. GEORGE'S STREET, CANTERBURY
(Block kindly lent by the Society for the Promotion of Roman Studies: see *J.R.S.* xxxviii, 99)

The earliest Roman phase in Claudian times, and subsequently under the Flavian dynasty, was embodied in timber buildings, of which sleeper beams and post-holes have here and there been found. The depth at which these have appeared has prevented wide excavation, and we know these periods in vertical section rather than horizontal plan.

Masonry replaced timber about the turn of the 1st century, and the ordinary houses were built in flint and mortar with corners turned in tile; but Kentish rag was used both now and later in more substantial structures.

No building has been recovered in its entirety, for they all either run out under a modern street or else disappear beneath a neighbouring undamaged property. It is interesting to see their relationship with more recent buildings. In two cases, the

CANTERBURY EXCAVATIONS, 1944-8

Butchery Lane house (FIG. 1, 3), and the house beneath the Rose Hotel Yard (FIG. 1, 8), they make no contact with their successors. In the Rose Yard the building had been well robbed in the late 4th century, and in Butchery Lane there was evidence to show that one Roman wall had been dug out in the later middle ages, while the rest had been cut down when the cellars were dug. But in three other cases things were different. In Burgate Street (FIG. 1, 1) one wall was still standing to within two feet of the surface, and had been in use throughout medieval and recent times though buried and encased; while the S. George's Street Bath-house (FIG. 1, 6) and the S. Margaret's Street Bath-house (FIG. 1, 9) both showed that the cellar-diggers of the 17th or 18th century, if they found a convenient Roman wall would build upon it, even if it meant taking a narrower line for their basement. This is well seen in the former site (FIGS. 2 and 3). The excavators of cellar A

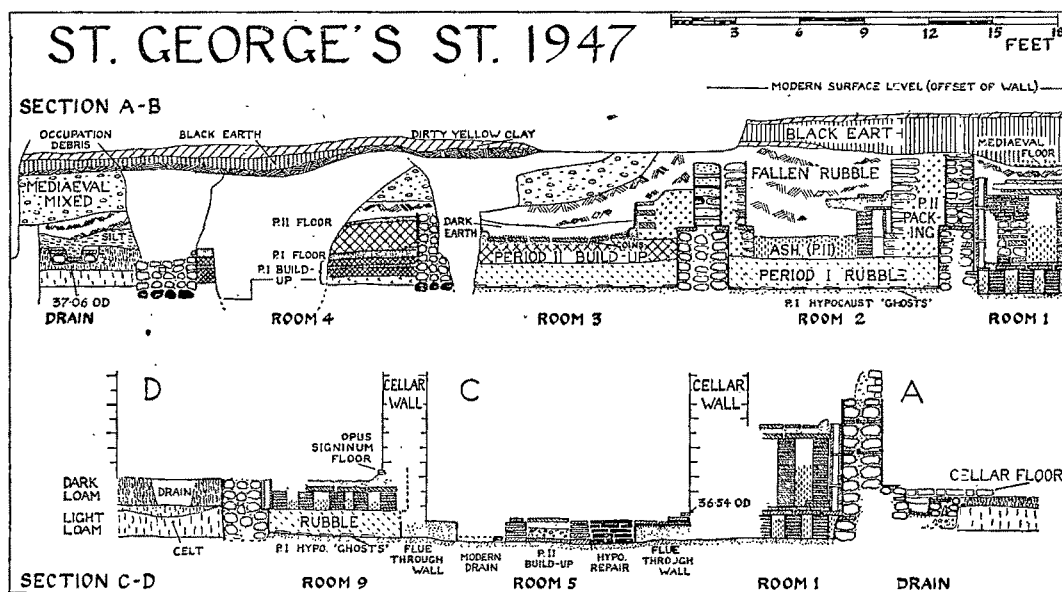


FIG. 5. SECTIONS OF ST. GEORGE'S STREET BATH HOUSE, CANTERBURY
(See FIG. 2)

struck the west wall of the Baths, still standing four feet high, and used its external face as the wall of their cellar, though this was four feet short of the property boundary. The men who dug cellar c cut through the second-period Roman hypocaust, the bricks of which they re-used in their cellar wall, still to be seen; and they used as their floor the tiled basis of this hypocaust, covering the blocked remains of its predecessor.

In cellar d, on the other hand, they did not sink so deep, and encountering a Roman *opus signinum* floor they re-used it. Unfortunately this floor rested on a hypocaust. Continual shovelling of coal wore down the concrete, and here and there holes appeared, leading to our discovery of unburnt coal in the Roman hypocaust!

The east wall of the Building crossed the neck of the cellar, whose wall then followed its line. Here, however, the excavators did not build on the top of the Roman wall, but masked it with their brick wall (FIG. 4). The reason perhaps was that it was not standing so high above their floor level as the west wall had stood in cellar A.

In the rear part of the site, north of cellar c, the building is still standing massively

ANTIQUITY

to within three feet of the surface except where destroyed by medieval pits. Indeed along section A-B the same is true, and the tiled floor of room 1 was only four feet down.

This leads us to wonder what was the appearance of Durovernum in Saxon times? One of the puzzling things about our excavations has been the curious absence of Saxon and Jutish objects. In many of the sections dug from the surface there is an abrupt transition from late Roman to 12th and 13th century deposits, with no trace of any intervening time. Until January 1948 our excavations had produced but one Saxon

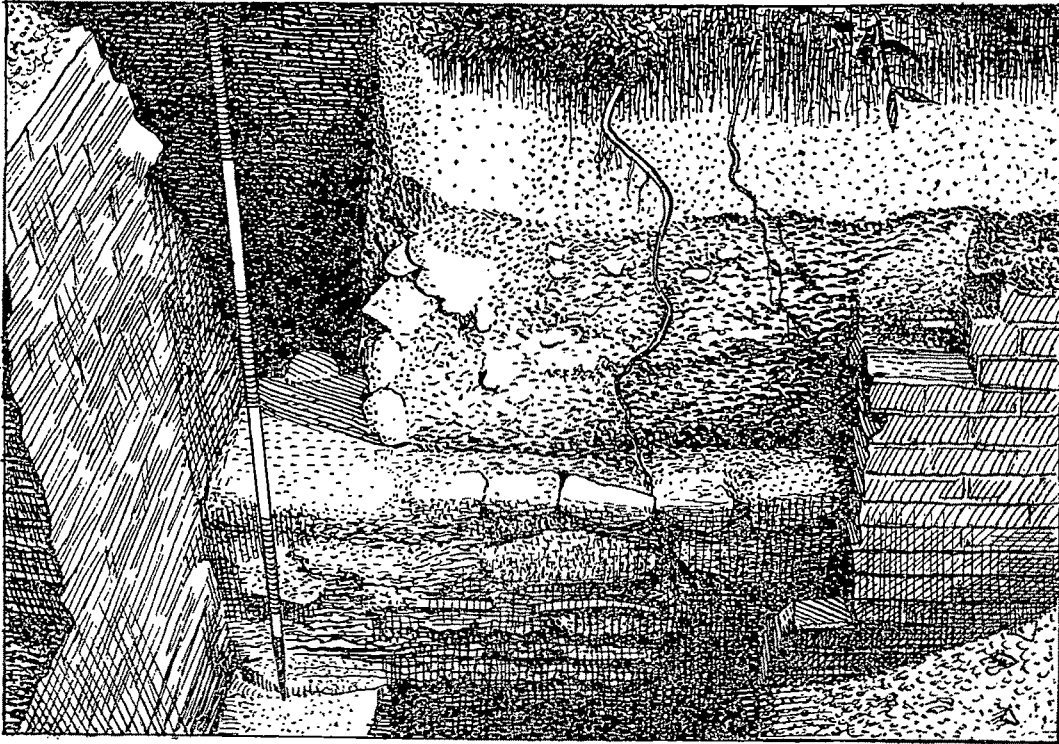


FIG 4. SOUTH WALL OF ST. GEORGE'S STREET BATH HOUSE MASKED BY MODERN BRICK WALL IN REAR PART OF CELLAR D; LOOKING SOUTHEAST

Drawn by B. Hope-Taylor from a photograph

sherd, though that of considerable importance. More recently two Saxon pits have been found, and a Saxon layer lying above the debris of one of the rooms in the S. Margaret's Street Baths. But on the whole it is true to say that Saxon levels are absent above the ruins of Roman buildings. Whether they exist in between the buildings remains to be seen, for little digging has been done except in cellars in such areas. There is convincing historical evidence for an extensive occupation within the walls certainly by the 9th century (3), and a straightforward reading of Bede (4) leaves little doubt that even in Ethelbert's time the nucleus of settlement was within the walls. If so, they must have avoided the gaunt ruins protruding here and there from piles of rubble; but if the

³ Birch, *Cartularium Saxonicum*, 515, 519, etc.; Stenton, *Anglo-Saxon England*, 519.

⁴ Bede, *Historia Ecclesiastica* 1, 25, 26.

CANTERBURY 1948

SECTION THROUGH DEFENCES

90 FEET SOUTH OF
RIDING GATE

SCALE : FEET

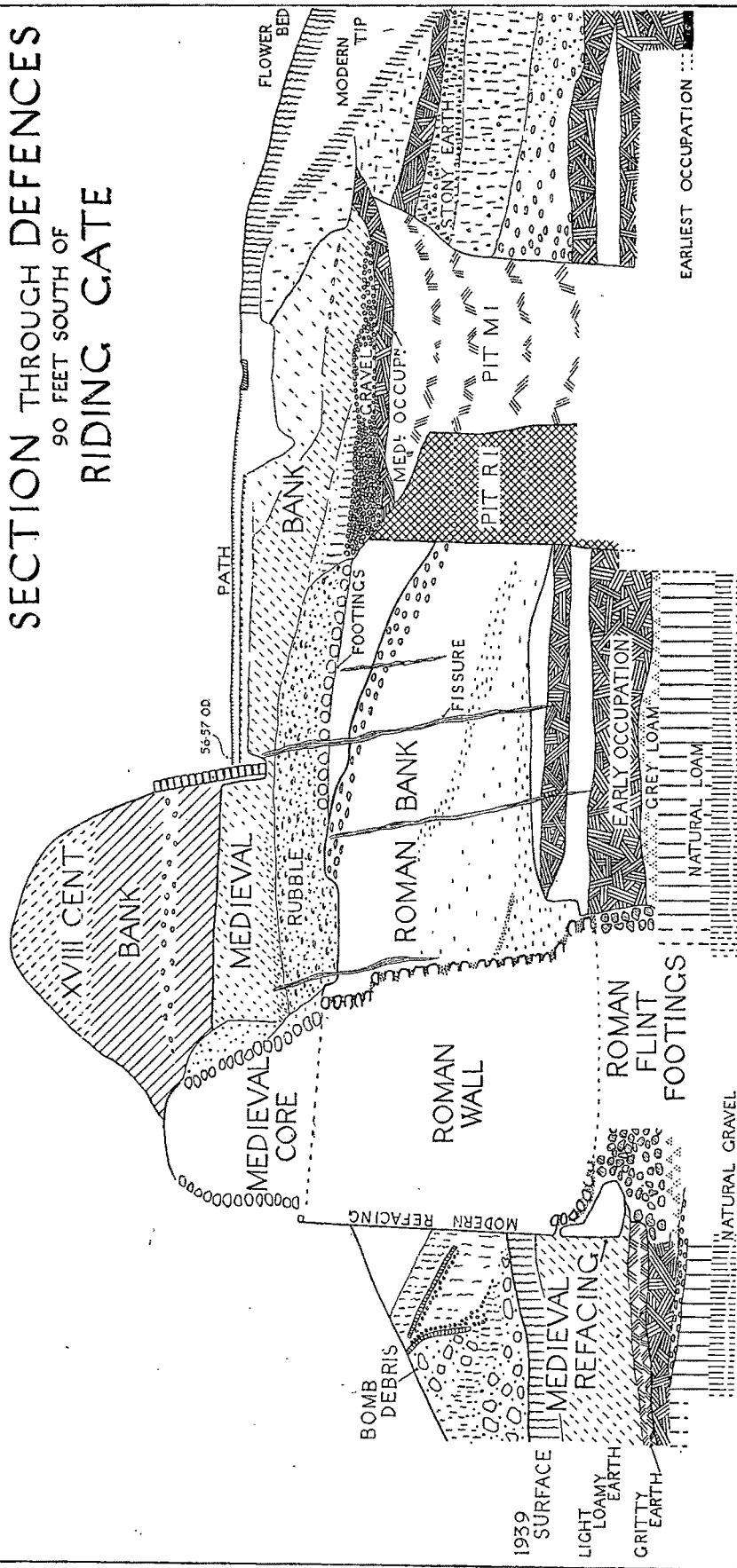
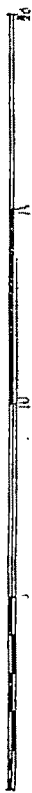


Fig. 5

ANTIQUITY

Roman town were never tightly packed with buildings, there would have been room enough for huts in intervening areas: and this perhaps explains why it was that the medieval streets lie over Roman houses.

THE DEFENCES

Until about a century and a half ago traces of three Roman gateways in the southern and eastern sections of the existing city wall attested the line of its Roman predecessor, and it was quite recently believed that the present wall itself was the work of the late 14th and 15th centuries (5). About the western sections of the Roman defences there was less certainty, but the presence of a chapel above the west gate in the reign of King Harold could be taken to imply a masonry gate on that site, and if masonry then doubtless Roman.

The present excavations have not yet dealt extensively with the problem of the defences, and the western part of the town is still unexamined. But in 1944 a section was cut just north of S. George's gate (6) revealing the Roman bank and the base of the medieval wall. The bank at this point was of clean loam six feet high, resting on natural soil. No dating evidence was found, but its absence could be taken to imply an early date. The medieval wall was dated to about A.D. 1300, a surprising result not reflected in the documents.

In 1948 a section was cut through the bank just south of the Riding Gate, and here again when the evidence is examined it will probably be found that the medieval wall was constructed about A.D. 1300. It may be that only the towers and gates were reconstructed at the later date indicated by the documents.

Unlike at S. George's Street, it was found here that the medieval wall had been built on top of a massive Roman wall of coursed flints and mortar, seven feet thick and still seven feet high (FIG. 5); and there was evidence that the medieval reconstruction was later than a small building on the floor of which lay several 13th or early 14th century jugs. This medieval floor rested on the summit of the Roman bank, a mound of predominantly cleanish loam capped with gravel. One is reminded of the cobbled rampart walk crowning the bank at York (7). The Roman bank itself contained much pottery, particularly in the dirtier tips, and this pottery is at earliest of mid-2nd century date; moreover the bank itself overlay earlier occupation layers, in one of which occurred a fragment of Castor ware. A date for this part of the Roman fortifications at the end of the 2nd century, possibly in the reign of Severus, seems provisionally probable.

This cutting threw much light on the Roman and medieval periods; but in truth it is a cross section of Canterbury's history in a fuller sense than that. Above the medieval refortification lay a late 18th century bank, constructed, as a neighbouring inscription testifies, in the years preceding 1802 and illustrating the rebirth of interest in the city's amenities at that time. Later still we have archaeological evidence of a great explosion sufficient to produce fissures in the Roman bank, and dated stratigraphically to the mid-20th century by deposits outside the wall, where twisted lumps of asphalt path and fallen wall lay above the pre-war humus; and this 'destruction-level' was succeeded by a period of calm before refortification, during which the acute angle between rubble and wall was silted up, and a turf-line formed. In the fullest sense a stratigraphical sequence from the earliest times to the present day!

⁵ *Arch. Cant.* XLV, 92-115, and D. Gardiner, *The Story of Canterbury Castle and the Dane John and its Manor*, 25-7.

⁶ By Mrs Audrey Williams, F.S.A., who was in charge of the excavations until January 1946. *Arch. Cant.* LIX, 69.

⁷ *J.R.S.*, xv, 177; xviii, 67-8 and pl. VI, 2.

Roman Coins found in Iceland

by HAAKON SHETELIG

THE State Antiquary of Iceland, Kristján Eldjárn, M.A., published in January this year a fine volume containing a report of his recent excavations of pagan graves, and other contributions to early Icelandic history, under the title of *Gergidh á Reika*, Akureyri, 1948 (1). It is very important that we have got here reliable accounts of systematically explored Viking burials, with diagrams and photographs, all very well done, as Iceland had produced, till now, very little of similar publications (2). But really exciting is undeniably the news of the discovery of three Roman coins in Iceland.

The place of discovery was at the farm Bragdhavellir, at the head of Hamarsfjörður, in the district of Sudhur Múlasýssel, on the southeast coast of Iceland. In a small valley called Djúpibotn the gales had partially denuded the ground leaving only the hard stony gravel subsoil. In this place the remains of two primitive houses came to light, certainly representing an ancient farm which had been deserted for long ages. A peasant of the vicinity Jón Sigfusson started searching the site for antiquities and collected a lot of such poor objects as are generally left in country dwellings of the early Middle-Ages, nails and fragments of iron, broken pots of soapstone, stone whorls, some teeth of horse and cow, bits of charcoal, etc. The only object of a more distinct character was a bead of variegated glass, reddish-brown with black and white, possibly dating from the Viking Period. Subsequently a number of the antiquities were sent to the National Museum in Reykjavík including two Roman coins said to have been found on the same site and under same conditions as the other articles.

One of the coins is an Antoninianus of Probus, found in 1905, and the other a similar coin of Aurelian found in 1933. The following year, 1934, the State Antiquary, Matthías Thórhðsson visited the site and explored the ruins of the houses. He found a number of relics of the same description as already mentioned, but could obtain no further precise and definite information about the discovery of the coins. He was convinced, however, that there was no reason to suspect intentional fraud.

In the meantime a third Roman coin had been found in an equally surprising way. An English teacher, Mr Leonard Hawkes, was in Iceland in 1923, and visited the same neighbourhood in Sudhur Múlasýssel. In passing the Hvalsdal, between Hvalsness and Krossanes, he accidentally discovered a Roman coin in the sand. He searched a little with the fingers, but found nothing more. The coin is an Antoninianus of Diocletian and is now preserved in the National Museum in Reykjavík.

The three coins date from the time of the emperors Aurelian (270-5), Probus (276-82) and Diocletian (284-305)—a period some 600 years before the Norse colonization of Iceland towards the end of the 9th century, and thus is raised a most interesting problem concerning the early navigation of the north Atlantic Ocean. The first question

¹ The title means roughly: Go on the shore of drift, i.e. to gather drift timber, or drift wood.

² Daniel Bruun og Finnur Jonsson, Dálvikfundet, *Aarbøger for nordisk Oldkyndighed og Historie*, København 1910, p. 62. Daniel Bruun, Udgravninger paa Island, *Geografisk Tidsskrift*, vol. 17, p. 3. København. cf. *Viking*, vol. 1, Oslo 1937, p. 205.

ANTIQUITY

to be answered is, then, whether the coins were brought to Iceland at the time when they were still the normal currency of the day, or at some later period. Matthias Thórdharson has expressed the opinion, after his examination of the locality, that the coins are not likely to have been left there in modern times, and should more probably be ascribed to visits of Celtic hermits or to the establishment of the first Norse colonists. In both cases the coins would have been first found as deposits in the earth in England and then brought to Iceland. The theory is not convincing, as the small copper coins had lost all appreciable value when they ceased to be accepted as current money (3).

Kristján Eldjárn rightly emphasize in the first place the fact that Roman copper coins are extremely rare outside the frontiers of the Empire, and adds the important observation that the three coins from Iceland are practically contemporary within a space of 30 years, of equal standard, and consequently represent the normal current money of the day in Roman territories. From this argument he concludes that the coins were brought to Iceland as the ordinary small change of Roman provincials, not as antiquarian curiosities from a past age. He points to contemporary Roman history as offering ample possibilities of explaining an accidental discovery of Iceland, e.g. the sea-power of Carausius as emperor of Britain, his fleets chasing Saxon pirates all round the coasts, and thus protecting also the commercial navigation in British waters.

Mr T. C. Lethbridge has kindly sent me his comments upon the coins from Iceland, recalling that there appears to have been serious trouble with the Picts about 306 followed by vigorous retaliation. During these several operations we might well imagine a Roman vessel storm-driven towards northwest as far as Iceland. The locality of the find on the southeast coast is perfectly consistent with this suggestion, which moreover provides a natural explanation of the find itself, and is thus stated by Lethbridge. I quote from his letter of 14 March.

'Had we however found a little collection of copper coins like the Icelandic find over here, it would certainly have been suggested some little votive offering made by a provincial Roman to a local genius. Several such little finds have been made in this district (of Cambridge). It does strike me as possible that the Iceland find might be compared with the kind of thing the old whalers used to do on the coasts of the Arctic. It seems to me quite possible that those Icelandic coins might have been put there by a Roman seaman who had been driven into the fjord in bad weather and who wanted to make some little offering, either as thanks for his escape or to ensure his safe return'.

If this theory be accepted, it supposes only a short visit on shore and satisfactorily explains why Matthias Thórdharson found no other Roman antiquities or antiquities of equal age during his investigation of the site. The lack of such associations was his chief argument for referring the deposit of the coins to the same date as the relics of habitation during the Viking period. But an occasional votive offering may well have taken place without leaving other traces of even temporary settlement.

The possibility of a Roman discovery of Iceland presents a highly interesting contribution to the early history of North Atlantic navigation. According to the Icelandic tradition the island was discovered about A.D. 850 by a Norse Viking sailing from The Faeroes to Norway and storm-driven towards northwest. He went on shore and stated that the land appeared to be uninhabited. Some 20 years later began the Norse occupation by emigrants from Norway, with considerable contingents from the Viking settlements in Scotland and Ireland. But the colonists soon discovered that they were not the first settlers in the Country, as they found in several places traces of primitive

³ Einar Ol. Sveinsson, *Landnám i Skaftafellsthingi*. Reykjavik 1948, p. 2.

ROMAN COINS FOUND IN ICELAND

dwelling with crosiers, bells and books left by Celtic monks and hermits. The Icelandic tradition is testified by many place-names, such as Papey (the priests' island), still extant, and by the Irish legendary literature. It is sufficient to recall the histories of St. Brendan and St. Ailbeus of the 6th century, the monk Kormac of the 7th century, and many others, and the testimony of Dicuil about 825, the first exact geographical mention with reference to first hand contemporary witnesses.

Mr T. C. Lethbridge, in his recent book, *Merlin's Island*, has adduced strong arguments for regarding the Norse discovery of Iceland as inspired by their Celtic precursors. Iceland was well known among the Celts in Scotland and Ireland at the time of the Norse occupation, and many of the emigrants started from the Viking colonies in Celtic surroundings. It is more than probable that they had here got some notions of the Celtic voyages to the distant island in the Arctic Ocean (4).

The Roman coins from Bragdhavellir now raise the question whether the Celts were really the first discoverers of Iceland, or if they might perhaps have been acquainted with ancient reports, once current among seafarers of Roman Britain, of the fantastic island, covered with ice and with flaming mountains, far away in the northern mists of the Ocean.

⁴ T. C. Lethbridge, *Merlin's Island, Essays on Britain in the Dark Ages*. London 1948.

Einar Ol. Sveinsson, *Landnám i Skaftafellsthingi*, has given a complete and excellent account of Icelandic place-names composite of 'papa', and a convenient review of Irish sources, as an introduction to his analysis of the Icelandic tradition of discovery and settlement.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- SOIERIES PERSANES, by GASTON WIET. Vol. 52, Mémoires de l'Institut de l'Égypte, Cairo, 1947. 251 pages, 24 heliogravures. [Chiefly about the textiles from Bibi Shahr Banu near Raiy (Rhages), with 16 new fragments and a golden ewer from the same site (10-11 c.). Good bibliography].
- CHATEAUX TURCS DU BOSPHORE, by ALBERT GABRIEL. Mem. Inst. Arch. Stamboul, vi. Boccard, Paris, 1943. [Review in AJA, July-Sept. 1948, 468-9].
- THE ITALIAN ELEMENT IN LATE ROMAN AND EARLY MEDIEVAL ARCHITECTURE, by J. B. WARD PERKINS: annual Italian lecture of the British Academy, 1947. [Proc. Brit. Acad., Geoffrey Cumberlege, Amen House, London, E.C.4: sold separately at 7s 6d net].
- ON SOME ROMANO-BRITISH PLACE-NAMES, by KENNETH JACKSON. *Journ. Roman Studies*, xxxviii, 1948, 54-8. [At last the origin of these names is being discussed by one who has mastered the language to which they belong].
- THE JOURNEYS OF CELIA FIENNES (1685-1703). Cresset Press, 1949. 9s 6d. [The diary of an intrepid lady who rode on horseback throughout the country, penetrating as far as southern Scotland, and recording many acute observations, some archaeological, some about food, wine and people].
- WSTĘP DO STUDIÓW NAD OSADNICTWEM WIELKOPOLSKI WOZESNO-HISTORYCZNEY: by WITOLD HENSEL. Tom. v, Biblioteka Prehistoryczna. Poznan, 1948. 224 pages, 77 text-figures. In Polish, without résumé. [About so-called prehistoric but actually medieval, fortifications, especially Slavonic ones, in Bohemia and Poland. Contains publication of much new material hitherto inaccessible on account of the war].
- DIE SCHWEIZ IN ROMISCHER-ZEIT, by FELIX STAHELIN. 3rd edn. Benno Schwabe, Basel, 1948. 659 pages, 205 text-illus., 1 map, 3 folding plans. 30 *Swiss francs*. [Revised and enlarged edition of the standard work on Roman Switzerland, combining archaeology and history, and listing every Roman find—a unique achievement].
- DIE TEXTILIENSAMMLUNG FRITZ IKLÉ-HUBER IM MUSEUM FÜR VOLKERKUNDE UND SCHWEIZERISCHEN MUSEUM FÜR VOLKSKUNDE, BASEL; by KRISTIN and ALFRED BÜHLER-OPPENHEIM. Vol. 78, Mem. 2, Denkschriften der Schweizer Naturforschenden Gesellschaft. Published by Fretz, Zürich, 1948. 267 pages, 159 illus., 3 coloured plates. [An analytical description of textile technique, with good diagrams; the ethnological and archaeological specimens in this famous collection provide the means of discussing a very large number of types. Good bibliography].
- DER LINDENHOF IN ZÜRICH, by EMIL VOGT. Orell Füssli, Zürich, 1948. 231 pages, 59 text-figures, 44 plates, 3 folding maps. 30 *Swiss francs*. [Account of excavations, 1937-8. Important also for the technique of excavation in a built-up area. An early Roman military settlement of pre-Haltern and pre-Oberaden period, with pottery. Late Roman fort of 4th c. Pre-Carolingian buildings. Carolingian Pfalz of 9th c. Pfalz of 10th c., and fortified Pfalz of 11th-12th c. Many reconstructions of buildings: very detailed report on finds].

Reviews

BRITISH ART AND THE MEDITERRANEAN. By F. SAXL and R. WITTKOWER.
Geoffrey Cumberlege, Oxford University Press, 1948. £3 3s.

This is an expensive scrap-book, where the prehistorian will find the old friends of his youth—the Folkton drums, Aylesford bucket, and Bath Medusa—doing their stuff once more. But he will feel some exasperation at meeting them again, for the book adds nothing to existing knowledge. It claims to be ‘a survey of the relation between the artists of Mediterranean countries and those of a country [Britain] beyond the Alps, from prehistoric times to the present day’. To do this adequately would require a range of knowledge beyond the attainment of the compilers. The help of specialists is acknowledged, and was obviously necessary. The term ‘artist’ is widely interpreted to include the makers of idols, town-planners and the builders of St. Paul’s Cathedral. We suppose this may be legitimate, but art thus studied in a vacuum, divorced from the society in which it flourished, loses most of its meaning. Surely however it is stretching the connotation too far to include a row of portraits of Roman emperors just because they ‘are known to have lived in, or passed through, Londinium’. Imperial progresses are seldom the source of artistic inspiration; and even if they were, these pictures would not show it.

The compilers would have done a real service if, instead of republishing old and familiar material, they had made and published photographs of objects still not at all, or inadequately, illustrated, such as the Marlborough bucket—a first-rate example of Late Celtic art. No scale is given for the illustrations, or for most of the plans, nor are there any indications of the sources, or references to published accounts; but many are already so familiar that this is unnecessary. But then, why publish them at all?

A map on plate xv purports to show the ‘distribution of stone churches’ (‘after S. Pfeilstöcker’), but omits one of the finest examples, that at Escombe near Bishop Auckland. The Roman theatre at Verulamium is illustrated (on plate xi) by an air-photograph so atrociously bad that at first sight we did not recognize it as such at all. Next to it is placed a photograph of the Albert Hall, whose inspiration is also Mediterranean. At the bottom of the page are Roman and 17th century English coins with the figure of Eritannia. These illustrations show how in 1672 and 1871 British artists were marking back to Rome. The other three illustrations on this plate show how the alphabet of Trajan influenced the builders of Jarrow in A.D. 685 and Eric Gill a few years ago. The same alphabet, we might add, formed the basis of the lettering designed for the 5th (current) edition of the Ordnance Survey one-inch map, the models being photographs of actual inscriptions. If it were not for two bad photographs this would be an interesting plate, at any rate for the non-specialist, if he doesn’t know the facts already.

Scotland has few direct connexions with Mediterranean art, but its one certain link—the vine scroll—is ignored. Many of the dates assigned are controversial. The proof-reading does not seem to have been properly done, to judge by the list of errata which itself contains an erratum—the emperor’s name was Antoninus, not Antonius or Antonininus!

ANTIQUITY

The preface contains a long list of those whose help is acknowledged. All of them are persons of distinction in their respective spheres, but we question whether their time might not have been better employed. Their names are not, however, attached to the sections concerned, and we are therefore left in some doubt about the degree of their responsibility. As we write, we have received a request to supply two photographs of Stonehenge for what would seem to be yet another book of a similar kind to be published abroad. The busy man is apt to regard the compilers of such books as an unmitigated nuisance.

In making an effort to supplement what has been said above by constructive criticism, one risks being told that a book should be criticized for what it is, not for failing to do what it does not aspire to. That would be a perfectly legitimate retort; but as the chief criticism we make is that this is not the sort of book which is most needed by students; it would seem desirable for us to give examples of books which are so needed. There are many such, but amongst them would certainly be a descriptive catalogue of all British inscriptions of the Dark Ages, with really good photographs of each, taken in the right light by an expert photographer. Very few such photographs exist, and the taking of them would involve field-work. But field-work helps the student more than a pair of scissors and a paste-pot, and is itself an enjoyable occupation. Difficult subjects, such as are some of the memorial-stones, are a challenge to the photographer, who has the artist's thrill of satisfaction when he secures a good picture.

Another book could be devoted to the fine lettering of the tombstones of the 17th and 18th centuries. Scotland is particularly rich in examples of such; the deserted graveyards of the lowlands must contain thousands, practically all of which are perishing unrecorded. An illustrated book would not only be attractive in itself, because of the beauty of the objects themselves, and of some historical value as well, but would also call public attention to a neglected and vanishing group of monuments. We are not aware of any book devoted to this subject, and of only a few articles, all indifferently illustrated.

The above suggestions assume that the publishers of a great University are fired with enthusiasm for the advancement of learning and willing to use their great resources to publish research which otherwise would remain unpublished; for the research worker is seldom able to pay for the publication of his work himself. But is this assumption valid? This book is, of course, published by Mr Geoffrey Cumberlege for the Oxford University Press, and not by the Oxford University Press itself; but the distinction is one that laymen can never quite grasp. O.G.S.C.

EARLY INDUS CIVILIZATIONS. By ERNEST MACKAY. *2nd edition revised and enlarged by DOROTHY MACKAY.* London: Luzac & Co., 1948. 15s.

This book, under the title of 'The Indus Civilization', was first issued in 1935 and has been out of print for some years. It is the best available summary of the remarkable Bronze Age civilization first identified in 1921 at Harappā (Punjab) but better known from the well-preserved remains subsequently uncovered at Mohenjo-daro (Sind), and its re-issue is more than an act of piety on the part of its editor, Mrs Mackay. The editorial additions are of no great extent; the main bulk of the work is as Mackay left it, and constitutes a useful, plain account of the remains then known, without any serious attempt at sociological interpretation. This omission might with advantage, be supplied in a future edition, since the material is now as abundant as it is likely to be for some years to come, and a careful assessment of its significance would be timely.

REVIEWS

In a purely constructive spirit, other suggestions may be offered for consideration in the preparation of the next edition. The revised title of the book, in its substitution of 'Civilizations' for Mackay's 'Civilization', is a misnomer. Only one Indus civilization is at present known—that which the editor rightly names specifically from the primary site, Harappā. Numerous other sites, mostly small, in Baluchistan and the Indus valley have produced heterogeneous industries, occasionally amounting to cultures but never attaining the status of 'civilization'. The confusion is perpetuated in the map facing page 1, where six 'Harappā' sites are associated without differentiation with five non-Harappā sites representing a variety of little-known industries and cultures. Incidentally, since no fewer than thirty-seven 'Harappā' sites have already been recorded and mapped elsewhere, the selection of a meagre half-dozen for the present map gives a very inadequate picture of the present state of knowledge. One of the more striking features of the Harappā civilization is its wide distribution, coupled with the outstanding size of Harappā and Mohenjo-daro, situated respectively in the northern and southern zones of intensity. These and other geographical factors invite discussion in the missing chapter on sociology, associated with a drastically revised map.

The greater part of the book deals with Mohenjo-daro, where Mackay himself dug for six seasons. A few references to Chanhudaro, the small but important site subsequently explored for a single season by the same excavator, have been inserted by the editor, without however giving any adequate indication of the evidence which it produced. Furthermore, in tabulating the culture-sequence of the site, the editor retains the illogical procedure of numbering from the top instead of from the bottom, so that her 'Harappā I' is later than her 'Harappā III'. A more logical sequence has already been proposed by Professor Stuart Piggott (*Ancient India*, I, 1946, p. 13) and is now used by students of the subject.

The type-site, Harappā, is very insufficiently described. A short and unintelligible account of the celebrated granaries there is concealed on p. 45, but no attempt is made to describe the remarkable and significant lay-out of the quarter in which these granaries are situated. I may perhaps be permitted also to regret that the massive citadel-fortification which I discovered in 1944 at Harappā, though at present unique and of obvious importance in the sociological assessment of the civilization as a whole, is not described or adequately discussed. Once more we miss the qualities of the constructive historical imagination which alone can bring these dead cities to life and give them meaning.

The section dealing with burial (pp. 73-4) is also obscure and incomplete. No mention is made of the fact that since 1937 a considerable part of a large Harappā inhumation-cemetery (R.37) has been uncovered at the type-site, clearly indicating the normal burial-rite of the civilization; and the omission is the more remarkable since one of the burials from this cemetery is in fact described and curiously noted as 'isolated'. Further, the reference to the well-known alien 'Cemetery H' at Harappa is so inadequate as to be meaningless. In all the circumstances, the retention of Mackay's view that 'the Harappā people usually cremated their dead on the banks of the river and that the ashes were then thrown into the water' is a manifest oversight on the part of the editor.

The illustrations (except the map) are generally satisfactory, and are more numerous than in the first edition. The old air-photograph, republished on a small scale as the frontispiece, is, however, almost valueless and would better be omitted; and plates I (i) and v show how an excavation should *not* be conducted. In short, the third edition will require extensive re-editing, after which the little book should continue to render useful service for a long time to come.

R.E.M.W.

ANTQUITY

SOME ANCIENT CITIES OF INDIA. By STUART PIGGOTT. *Oxford University Press*, 1945. 5s.

This admirable little book was written in 1944-5, primarily for the many military and air-force visitors then in India. But in spite of its limited scope and purpose it has a permanent value as a fresh and well-written account of ten important sites, with a concise historical introduction and a chapter on prehistoric western India. This chapter deals mainly with the Indus or Harappā civilization, and in its broad lines might serve as a model for the chapter of which I have regretted the absence in my note on Mackay's book (above). Incidentally the air-photograph which illustrates it presents an intelligible picture of a part of Mohenjo-daro, although reproduced with too coarse a screen.

The chapter on Taxila is a succinct summary of that important and complex site, and its illustrations include an unpublished plan of the most elaborate building known from the earliest city (the Bhir Mound)—a large courtyard structure with a pillared hall, dating approximately from the time of Alexander the Great. Chapters on Sanchi, Ajanta, Ellora and Abu are lively summaries of the rich architectural, sculptural and pictorial features of those Buddhist and Jaina sites, and a selection of the innumerable monuments of Delhi is briefly described. Mathura has given the author a more ample opportunity for new work, and the sketch-survey of the extensive town walls is an addition to knowledge. Daulatabad, in the Nizam's Dominions, has also yielded new material, and the sketch-plan of this remarkable series of 14th century defences is the first published. Finally, two chapters deal with the famous Moghul complex at Fatehpur Sikri and Agra, concluding with an intriguing collection of opinions on the aesthetic value of the Taj Mahal.

The book as a whole shows how much can be achieved even by a hurried visitor (such as Professor Piggott would admit himself to have been) provided that he possesses a trained and discerning eye and an expressive pen.

R.E.M.W.

THE ANNUAL OF THE AMERICAN SCHOOLS OF ORIENTAL RESEARCH.

Vol. XXIV for 1944-1945, *Published under the Jane Dows Nies Publication Fund*, 1947.

Seal Impressions of Nuzi, by EDITH PORADA, PH.D. pp. 138, plates LIV.

This volume contains a selection of more than one thousand of the impressions on the tablets found at Nuzu, most of them in the house of a certain Tehip-tilla. The majority of these impressions is attributable to four or five generations of one and the same family, well-to-do citizens who were living at Nuzu in the fifteenth century B.C. The population at that time was predominantly Hurrian and appears to have been ruled by the king of Mitanni. Most of the impressions reflect the 'popular' style of glyptic which was widely diffused in Mesopotamia, Syria and Palestine at that period. This very rich collection is therefore an invaluable landmark in the history of ancient glyptic, since it enables us to pin-point a large number of different cylinders to a precisely measured time-sequence. There are also some earlier impressions which go back to the Jamdat Nasr period. Miss Porada has made an elaborate and detailed study of the evidence and has examined the comparative literature very carefully. This volume will take its place as a standard reference work for this period of Mesopotamian glyptic; the text contains much instructive comment on a variety of problems, many of which are not yet fully solved.

M. E. L. MALLOWAN.

Antiquity

VOL. XXIII No. 92

DECEMBER 1949

Editorial Notes

AT the end of the year it is natural to look back and take stock. What have archaeologists achieved in 1949 and how far has ANTIQUITY succeeded in reflecting those achievements? The two most important events of the year were the publication of a discovery made last year in Palestine, of a cache of Hebrew scrolls of the Old Testament, and the invention of a new technique by means of which ancient fragments of wood and other organic substances can be dated in terms of years. We gave our readers the essential facts about the Hebrew scrolls long ago (September, 1948, pp. 159-60), some time before their importance was generally recognized; and we did the same for the radiocarbon method in our last number. We hope to publish more about both. As so often happens, neither discovery was made by archaeologists; the scrolls were an accidental find, and the radiocarbon method was discovered by physicists. The importance of both lies in the realm of technique rather than in any new historical knowledge revealed. That is obvious for radiocarbon; for the other it seems that the chief contributions will be linguistic and palaeographic—of immense value and far-reaching importance but primarily technical.



The exact date of the scrolls is not yet finally determined. They were stored in pottery jars with saucer-like covers of a Hellenistic type already known and dated a century or thereabouts B.C. As Mr Lethbridge pointed out in a letter to the *Times*, it is absurd to suppose that antique jars, complete with their antique covers to the number (originally) of 30 or 40, would have been used; and the date when the cache was made must therefore be that assigned to the jars—and this is a purely archaeological affair. The jars date the texts.



There are evident signs that British archaeology has got into its post-war stride. The dry summer has been welcomed by air-photographers and excavators. Dr St. Joseph has made some more remarkable discoveries, this time in s.w. Scotland, and we shall look forward to the annual exhibition, organized by Kodak Ltd., with more than usual interest. (Why not have one in the winter?) In this connexion we would suggest that an exhibition of British archaeological air-photographs should form part of the Festival of Britain in 1951. This is a line we started in this country and have made our own, though the French, led by the Rev. Father Poidebard and now also by M. Baradez are now becoming close competitors in Syria and North Africa. (When will they begin

ANTIQUITY

work at home? France offers a fine and virgin field). Drought has also helped Dr Grahame Clark in his excavation of the first inhabited mesolithic site (if we except his mesolithic camping-ground near Farnham, Surrey) to be discovered and scientifically excavated in this country (see pp. 207-8 of this number). Professor Stuart Piggott has excavated a chambered Long Cairn (Cairn Holy in Kirkcudbrightshire), and found a partly intact burial-chamber. One of the finds was part of one of those greenstone ('jadeite') axes with a highly polished glassy surface which are usually thought to originate in Brittany. Thus they can at last be safely dated, and the already long-suspected migration northwards along the Atlantic seaboard is confirmed in a striking manner. Excavations have continued on the sites of Saxon towns at Thetford and Southampton, and of Roman London and Canterbury; a Roman villa at Lullingstone has produced a very fine pavement and statuary. An ancient homestead site at Mynydd Bychan in Glamorganshire has been excavated. In Ireland, Professor Bersu has proved, by excavation, that hill-forts do exist there, and has found evidence of extensive iron-working in his hill-fort near Kilkenny. (The list is of course incomplete and we apologize for the omissions).

Many of these excavations have been carried out by voluntary student labour. The first-hand experience thus gained must necessarily be valuable; even more so perhaps will be that of those taking part in organized courses. There have been at least two such; one, conducted by Professor Wheeler at Verulamium; and the other by Professor Piggott at Salisbury. The latter was organized by the British Council, whose new enterprise is warmly to be commended. It was designed for advanced students and included archaeologists of known standing and achievements. As was only to be expected, the course followed in the footsteps of General Pitt-Rivers, and aimed at displaying not only the field-works (to coin a phrase from 'field-archaeology') which have survived better here than abroad (hill-forts, Celtic fields, etc.) but also the history of the subject, its organization (by State, Universities and Societies) and techniques (air-photography, excavation, field-work, electrical sub-soil survey). The Archaeology Officer of the Ordnance Survey (Mr C. W. Phillips) gave a lecture on 'Archaeology and the Ordnance Survey'. The success of the course would amply justify its repetition in 1951. May we suggest that representatives of archaeology in the Americas should be specially invited to take part? They would be sure of a hearty welcome from their British colleagues.

Another outstanding feature of the year has been the meetings of the Prehistoric Society. A three days' conference was held at the Institute of Archaeology in the spring and another at Exeter in the autumn. Both were in delightful surroundings, very different from the stuffy atmosphere of Burlington House, and there was 'a vehement spirit of research in the air'. The first meeting was devoted to prehistoric houses, a subject which the President, Sir Lindsay Scott, has made his own. It was an excellent idea to have such a single subject instead of a set of disconnected addresses; it gave coherence to the discussions which were spread over the meeting, and incidentally saved valuable time, because it was thus possible to concentrate into a single discussion the remarks about two or more addresses. The general level of discussion was a high one, controversial points were dealt with in a manner befitting a scientific society; and when it was all over we all felt that we had not only had a good time but also an edifying one.

EDITORIAL NOTES

In this, the last number of the year, we have to remind our readers that their subscriptions for 1950 are due, and that prompt payment is a very great help. Forms of payment for 1950 are inserted in the copies of all who do not pay by Banker's Order. May we urge those who do not do so already to *pay their subscription in future by Banker's Order*, a form for which is included in the present number? This saves a great deal of work at our end, and will also save them some. It of course involves no obligation beyond the current year. May we also suggest that ANTIQUITY for 1950 would be a very good Xmas present? Many readers have in the past thought this a good idea; the beneficiaries have generally agreed to become subscribers for the following years. The form makes provision for this also.

Will our American readers please note that devaluation has reduced their subscriptions to 2 dollars? And may we suggest that they might spend the balance, which is nearly one half, on a Xmas present? We shall, of course, give the same, or better, value for it.

We would also ask indulgence of those readers who may receive publicity leaflets during 1950. We plan to send out a large number, and although the Editor tries to eliminate known subscribers, he cannot carry in his head the names of all; and mechanical collation is costly and impracticable. If you do receive such there is a very simple remedy—send it on to a friend! The results of our 1949 campaign were very satisfactory—so much so in fact that we had to stop prematurely because we ran out of stock. On the strength of it we have been able to add an extra eight pages; this is only a beginning. If all goes well we shall be able gradually to recover our former size and abundance of illustrations. No one can do more to help achieve this than those readers who get others to subscribe; one such all round (as a Xmas present?) and we should reach the goal at one bound. We are determined to get back somehow to our former size, if possible by increasing our circulation and not your subscription, which—be it noted—is the same as in 1927 when we began.

It is because we are so terribly cramped for space that the Review section has shrunk so much recently. Actually in the present number we have expanded it considerably, but we need two or three times as much space to publish what we already have, let alone reviews of the new books that are constantly arriving. This is a problem common to all periodical publications nowadays. We apologize yet once more to all concerned for the delay, or even failure to review at all; lack of space is wholly and solely to blame. The new section listing Important New Books and Articles is an attempt to mitigate the situation. Again an increased circulation is the only remedy. But we do not wish to end the year with a moan! We have complete confidence in the future, and of our ability to provide our readers with good fare.

As we go to press we have received a very interesting letter, announcing the first archaeological date provided by the radiocarbon method. As there is no room for it on this page we have printed it on p. 229, at the end of the Review Section. Readers in America will be particularly interested.

Moas and Man

(PART I)

by R. S. DUFF

Director, Canterbury Museum, New Zealand

WHEN we remember that the Maoris volunteered no traditional information about the extinct moa (*Dinornis*) until Europeans had unearthed its bones, said nothing about the Chatham Islands until after their discovery by Europeans, only recalled dim memories of inhabitants before the Fleet of A.D. 1350 in response to persistent questioning by Europeans, and could not tell us whether Hawaiki¹ was Tahiti or Samoa, we realize the always supine rôle of Maori tradition in aiding the researches of the culture historian.

However the sheer mass and variety of these orally transmitted traditions prevented the student from realizing how irrelevant they were to his theme, and caused him to believe that the Maori purpose in transmitting traditions was like his—to satisfy an essentially academic curiosity about the past. The gradual cessation of the output of published traditions has given students the leisure to realize the limitations of those already recorded, and sobered us against the expectation that a Maori tradition current in the 19th century might include a description of a bird which lived perhaps in the 13th, or go into detail over the appearance and habits of the tribes whom his Fleet ancestors dispossessed in the 14th.

Fortunately the need for the family to maintain its status within the clan, the clan within the tribe, and the tribe as against other tribes, did involve the careful transmission of family trees (Whakapapa). By comparing the number of generations in many lines back to a Fleet ancestor, the arrival of the Fleet was placed in the mid-14th century. By a brilliant application of the method beyond New Zealand, Percy Smith found a three generation name sequence immediately prior to the Fleet arrival common to Hawaii, the Society Islands, the Cook Islands and New Zealand. This established with reasonable certainty that the movement which brought the canoes of the Fleet to New Zealand originated in the Society Islands and simultaneously sent migrants to the Hawaiian and Cook groups. Traditions in New Zealand recorded with a significant unanimity the names of the canoes of the Fleet migration, their landing places, and the tribes which sprang from each. They noted the introduction by the immigrants of the sweet potato (kumara), the taro (*Colocasia antiquorum*), the gourd (*Lagenaria*), and the yam (uwahi), both by means of references to incidents of the voyage or by accounts of subsequent return trips to Hawaiki to fetch these plants.

Beyond this era little information was volunteered in tradition until the need to establish court claims to land brought belated mention of family trees going back to an

¹ By Hawaiki is meant the Society (and in transit the Cook) Group which were reasonably established by comparative genealogies as the immediate origin of the Maori Fleet, Toi and Kupe. Traditional evidence had eliminated the claims of the Samoan Group in Western Polynesia.

MOAS AND MAN

earlier Hawaiki immigrant, Toi, whose arrival was placed about two centuries prior to the Fleet, *circa* A.D. 1150. Christened the 'first born' (Toi-te-huatahi) in Hawaiki, Toi was remembered through the Fleet traditions of 1350 as the man 'who lived on wild [lit. tree] foods' (Toi-kai-rakau), a significant reference to the failure of his migration to introduce crops.

Beyond Toi, there are no reliable family-trees, and only confused and conflicting references to the existence and place of origin of prior inhabitants. Regarding the pre-Toi inhabitants one plausible, circumstantial and widely publicized account (S. Percy Smith, 1915) has been unfortunately taken as indicating a non-Polynesian migration which students have eagerly invoked in explaining any and every point of difference between the culture of the Maoris of the late 18th century and their Polynesian relatives.

Finally, as there must be a discoverer belonging to the Hawaiki stock to which the Fleet ancestors belonged, New Zealand's discoverer was alleged to be Kupe, *circa* A.D. 950. We are told, improbably, that after discovering New Zealand and exploring its coasts, Kupe returned to Hawaiki and left no settlers. However Kupe's sailing directions were handed down in Hawaiki and proved precise enough to enable Toi, two hundred years later, and the Fleet, four hundred years later, to sail into the blue, reach and settle New Zealand.

Summing up the traditional evidence, the sequence of events is as follows :—

The Hawaiki Polynesian, Kupe, discovers New Zealand, but returns and leaves no settlers ; A.D. 950.

A non-Polynesian migration (Maruiwi or Mouriri), coming from the northwest, settles the North Island ; date uncertain, but post-Kupe.

The Hawaiki Polynesian, Toi, rediscovers and settles New Zealand ; A.D. 1150.

The arrival of the Hawaiki Fleet brings to a close a general period of migration from Polynesia. Introducing the sweet potato and other food-plants, the newcomers impose themselves as an aristocracy upon the Toi and pre-Toi descendants, and found the tribes which were dominant in Cook's time ; A.D. 1350.

After the Fleet arrival, intercourse with Polynesia ceased, and New Zealand's isolation was not disturbed until the arrival of Tasman 1642 (not remembered in Maori tradition) and Cook (1769).

This sketchy framework of Maori origins is unlikely to receive any new elements from tradition, and the first task of archaeology is to endeavour to answer some of the questions regarding the pre-Fleet settlement which are left unsettled.

These are :—

- (1) Whether the human settlement of New Zealand is likely to be earlier or later than the traditional date of A.D. 950.
- (2) Whether any non-Polynesian migration has reached New Zealand.
- (3) Whether the presence of moa remains in middens fixes the deposit as a pre-Fleet culture stratum.

Dealing with the last first, the presence of bones of the moa, and, as we now know, other extinct birds, in human middens provides for the New Zealand archaeologist an obvious indication that the site is old. How old archaeology in itself cannot tell, because within a time framework of one thousand years or so an occupation layer of the first two centuries need look no older nor be bedded deeper than one dating from the two centuries immediately preceding European settlement. The depth of deposit bearing soil is normally less than three feet, while stratification virtually does not exist.

ANTIQUITY

SIGNIFICANCE OF PRESENCE OF MOA BONES IN HUMAN DEPOSITS

The first fact to establish was indeed the basic one, whether the moa was contemporary with man, when, in a country widely endowed with surface deposits of sub-fossil moa-bones, the evidence comprised finding moa-bones and human ovens in conjunction. To this, geological doubts as to the survival of the moa beyond the post-glacial period and the extraordinary vagueness of Maori traditions, contributed.

The first discovery of quantities of moa bones in human middens was made at Waingongoro, on the south-west coast of the North Island, as early as 1843, while four years later, at Awamoa, Mantell made the first of numerous discoveries from the east coast of the South Island. However, as Colenso noted as early as 1841 (Colenso, 1843) that the Maoris in the East Cape district of the North Island were in the habit of collecting sub-fossil moa-bones for manufacture into fish-hooks, the presence of moa bones in a midden could not of itself be taken as proof of the killing and eating of the birds. The presence of stout limb-bones in sites where there is no traditional possibility of the bird having been a contemporary is now widely established, while Steele's experiments have proved sub-fossil bone to be sound enough for all the uses to which recent Maoris could have put it.

Even the discovery at Kaikoura in 1859 of a moa egg in an apparently Maori grave did not settle the issue beyond doubt. Many of those who opposed the views of Julius von Haast (1872) that the moa could only have been a contemporary of an ancient 'autochthonous' palaeolithic man in New Zealand shared his opinion on the egg when he 'could not conclude therefrom that the . . . egg was of co-temporaneous origin with the individual with whom it was buried . . .'. Evidence of the now well-established custom of perforating the eggs from one end only, was not recorded, and now that the egg is in the possession of a collector at Anglesey, ANTIQUITY might be able to render a service to New Zealand archaeology by publishing photographs of this long-elusive exhibit (2).

This doubt persisted through the subsequently discovered sites (which were all at other points dotted along the east coast of the South Island) until the discovery in 1939 of the now famous site at the mouth of the Wairau river. The recovery from the Wairau graves of no fewer than ten moa eggs, of which the few sufficiently intact to reveal it had been blown by means of a perforation at one end, finally established beyond doubt that the moa and man were contemporary, and that the moa remains in these camps were true midden deposits.

Furthermore, by revealing a material culture demonstrably distinct from recent Maori culture, the Wairau excavations greatly strengthened the hypothesis, already justified by the negative evidence of the absence of traditions, that the moa was exterminated in pre-Fleet times. As stated earlier the absence of sufficient references to the moa in recent Maori traditions is not in itself sufficient to justify the inference that the bird had been exterminated by pre-Fleet tribes. Taken in conjunction with the recovery of a quite distinct material culture from the largest Moa-hunter camp yet excavated, the absence of traditional evidence becomes much more significant.

Until sites where moas were killed and eaten reveal artifacts of recent Maori type it is justifiable as a working hypothesis to assume that the presence of bones of Moas and other extinct birds fixes the deposit as a pre-Fleet stratum. For the North Island this hypothesis might be categorically applied as indicating a pre-Fleet stratum in point of time. For the South Island, more remote from the reach of new migrations from

² Our efforts to secure this were unsuccessful.

MOAS AND MAN

Polynesia, and in its dry eastern grasslands more thickly populated with moas, the possibility of moas and Moa-hunters overlapping the Fleet arrival until the late fourteen hundreds must be taken into account. While there is in fact no traditional justification for a later survival of moas in the South, I feel justified by caution in fixing the year 1500 as the effective termination of the Moa-hunter era in the South and the Fleet arrival of 1350 as its certain termination in the North.

Field workers must still of course exercise sufficient care to establish that the association of bones and middens is primary, that it is not for instance the result of a common type of wind erosion in coastal sand-dunes by which oven stones and implements finally come to rest on pre-human moa remains, nor confined only to stout and durable limb-bones imported for fashioning into implements.

Up to now most Moa-hunter camps and all important Moa-hunter finds have been made in the South Island; the North Island can only establish a probable contemporaneity of moa and human remains at Opito in the Coromandel Peninsula and at Porirua and Waingongoro on the south west coast, overlooking Cook Strait and the South Island.

For this contrast the originating cause was the greater suitability of the extensive grassland zone on the east coast of the South Island as a habitat for the most common (if not the only) moa which survived into human times. This was *Euryapteryx gravis*—a massive but comparatively short-bodied genus standing between five and six feet high. But the scarcity of North Island records is also attributable to other factors, including its readiness of access to all arrivals from Polynesia and the probability that the first tribes were swiftly succeeded by others, and, no less important, the continued presence there today of a large Maori population hostile to archaeological projects. Apart from the larger moa population there, the South Island represented a remote or marginal area in which the first settlers remained in much greater isolation and survived long enough to have left large and easily identifiable occupation sites.

If this thesis is valid, New Zealand is unique in Polynesia in that the beginning of the process of local culture development can be fixed by the objective evidence of the presence of moa bones and compared on one hand with the first local development of Maori culture, while providing on the other the most direct evidence yet on the prototype Polynesian culture of the Eastern Polynesian Hawaiki.

BEARING ON DATE OF DISCOVERY OF NEW ZEALAND

I return now to the first question which tradition leaves open, whether the discovery of New Zealand is likely to be older or later than the accepted date of A.D. 950.

The starting point for the discussion is the demonstration that moa remains, which in some South Island sites cover as many as 150 acres, were wholly from birds killed by man. At Wairau it is not possible to dig anywhere over an area of 15 to 20 acres without striking bones; at Rakaia, Haast found them scattered over 50 acres, while, at Waitaki, Teviotdale records an area of 150 acres. While on the last two sites the bones were scattered rather than heavily concentrated, at the much smaller Shag River site (an area less than 300 by 70 yards) the bones were so numerous that, as Skinner records 'The next investigator . . . dug up Moa bones, conveyed them on a punt constructed for the purpose, up the Shag river to the railway, and filled several railway trucks with them. These were dispatched to Dunedin bone-mills . . .'

If these river-mouth deposits represent a fair sampling of the moa population then distributed over the whole eastern face of the South Island, the birds must have been

ANTIQUITY

virtually as numerous and widespread as the domestic stock of today. Assuming also that the traditional evidence correctly assigns their extermination to the pre-Fleet period, this would imply a human occupation commencing much earlier than the four centuries traditionally allowed, from Kupe's discovery in 950 to the Fleet arrival of 1350. However I suspect that the river-mouth deposits in some way represent an abnormal concentration, their significance being due either to the preference of the surviving genus (or genera) for an estuarine habitat, or to the use of water transport by the Moa-hunters to bring them downstream from inland haunts. Regarding the range of genera which survived into the human period, R. A. Falla (1942) concludes on good evidence that only one (*Euryapteryx gravis*, 'the quarry par excellence of the ancient moa-hunter') survived in large numbers, while there were occasional remains of *Emeus huttoni* and some evidence for *Pachyornis*. This dwindling of the moas to virtually one genus would in my opinion support the presumption that this genus (*Euryapteryx*) was not evenly distributed but concentrated in certain favoured localities; but even granting this hypothesis it obviously survived in enormous numbers in these favoured spots.

Perhaps even more significant than the moa evidence, is the demonstration from middens at Wairau and at Sumner that the Moa-hunters exterminated a swan (*Chenopsis sumnerensis*) whose existence is completely forgotten in Maori tradition. From the Chatham Islands where the same bird had also been exterminated, the extermination was recent enough for the name (Poua) to survive, also a detailed memory of a method of taking the moulting birds by driving them over the lagoons into V-shaped corrals of brush-wood; but the Morioris had retained no clear notion of the appearance of the bird itself, so that until he found its bones in 1892 H. O. Forbes (1893) was eagerly expecting a new genus of *Dinornis* or *Apteryx*. Assuming *Chenopsis sumnerensis* was as well adapted to estuarine conditions as its recently introduced Australian relative (*Chenopsis atrata*), one must agree with Forbes that the 'cause of the total extinction . . . of the ancient swan . . . in its natural home appears at present inexplicable'.

The extinct eagle (*Harpagornis*) whose bones, worked into awls, survived at Wairau and Sumner to remind us of this third fabulous contemporary of man, occupies in Maori tradition a position almost as shadowy as the swan, little more than the names (Hokioi and Poua-Kai) being remembered. In the latter we may recognize, in the light of the Moriori traditions, a translation of the Swan.

Archaeology has then demonstrated the extermination by man of three bird species, of which the largest and most spectacular (the moa) was not sufficiently remembered in tradition, and what appears to have been the most numerous (the swan) was not remembered at all; while traditions of the eagle are so fabulous that they may in fact be distorted memories of either the moa or swan. The most convincing explanation of the absence of tradition is that the extermination was effected by the predecessors of the Fleet immigrants, and the material culture of Moa-hunter camps supports this assumption. Accepting 1350 as the date of the Fleet arrival, are the four centuries allowed beyond that date to the quite hypothetical discoverer Kupe sufficient for the extermination of three species of birds of which the moa and swan must have existed in vast numbers, with the latter and the eagle presumably difficult to capture? We can regard it as certain that the pre-Fleet tribes did not possess the bow, or any missile hunting weapon.

Summing up his review of the bird remains in Moa-hunter camps, R. A. Falla emphasizes the possibility that the date of human discovery should be pushed back to allow the early migrants sufficient time to have exterminated the moa, the swan, the eagle and other extinct birds. 'The traditional dates between A.D. 900 and 1150', he writes, 'are currently accepted as correct for the first Polynesian immigrants. This

MOAS AND MAN

leaves only two or three hundred years, or at most five hundred in the South Island, for a marked change not only in the personnel and culture of the human inhabitants, but also in the composition of the avifauna. Either the arrival of the first human immigrants must be regarded as having taken place more than a thousand years ago, or we must conclude that the end of the era of the moa, the eagle, the crow and the swan in New Zealand has occurred somewhat abruptly within the last thousand years'.

The final solution here is likely to be provided by Zoology. Archaeology has however supplied the evidence for the present zoological formulation of the problem, which represents an immense advance on the subjective basis of previous speculations, and should in time provide the additional decisive evidence.

In the meantime there is strong circumstantial evidence for believing that human settlement must be earlier rather than later than A.D. 950, while it might be categorically stated that Toi's migration (1150) cannot represent the first occupation of New Zealand.

THE PROBLEM OF A NON-POLYNESIAN MIGRATION

In the light of archaeological investigation, particularly of the Wairau site, to which this essay serves as an introduction, the possibility of any non-Polynesian migration reaching and establishing itself in New Zealand is seen as improbable.

It should be emphasized that traditional support for such a migration dates only from the present century, when in 1913 the Polynesian Society published the second of two volumes of a Maori manuscript allegedly written down in the eighteen sixties. The origin and authenticity of these traditions have been severely criticized by Williams (1937), while no one has established from the somatology of the Maoris of today, or from skeletal and archaeological remains, any greater incidence of Melanesian characteristics than in other parts of Polynesia.

Regarding this improbable hypothesis of a Melanesian migration, it is certain that it has not arrived since the Fleet, nor would even the most partisan Maori tradition allow it to follow any Polynesian settlement, so that its effects should be stronger the earlier we go back. To establish that the earliest demonstrable phase of New Zealand culture differs from the recent phase, not in the existence of Melanesian affinities but in the more obvious incidence of Eastern Polynesian characters, causes us to look first to local development to explain features of recent Maori culture which do not readily accord with an Eastern Polynesian origin.

For want of a better term I propose to revive Haast's original term Moa-hunters to describe those settlers who began and probably completed the extermination of the important group of avifauna described, in the certainty that some of their camps must be of pre-Fleet date and in the possibility that all may so be. The evidence is largely from the east coast of the South Island, but as many of the distinctive artifacts have been found in the extreme north of the North Island and most in the Chathams, the presumption that this is New Zealand's oldest, and originally a widespread, culture is strong.

The idea that the Mōa-hunters were in physical type and culture quite different from the Maoris dates from Haast's letter to the Zoological Society in 1870 on the Rakaia camp. Haast's original Moa-hunters were earth-born ('autochthones'), living at a remote period when a land bridge joined the two islands of New Zealand and, like the ancient hunters of Europe, whose existence had been recently demonstrated by Boucher de Perthes, palaeolithic. Haast finally agreed that he was mistaken on this last point, but he did not cease to regard the Moa-hunters as preceding the Maoris by thousands rather than hundreds of years. The next 'red herring' to be drawn across the

ANTIQUITY

Moa-hunters' trail was the view, largely disseminated by S. Percy Smith (1915) and Elsdon Best (1916), on the basis of the suspect Maruiwi tradition already referred to, that the earliest inhabitants of New Zealand were an inferior Melanesian migration from Fiji or the islands westward, who were conquered by the superior Polynesian immigrants of the Hawaiki Fleet of A.D. 1350. These views, the first over-emphasizing the remoteness from the Maoris of the period and type of the Moa-hunters, and the second mainly a fabrication, had their expected reaction in an over-emphasis of the identity of Moa-hunters and Maoris.

The main emphasis for instance in New Zealand's first sustained and competent archaeological project, the careful excavation of South Island coastal sites from the Waitaki river to the Tahakopa river, was the negative one of rejecting any evidence of a non-Polynesian migration (Teviotdale, 1933). Inspired and directed by Cambridge-trained H. D. Skinner, lecturer in ethnology at Otago University College, David Teviotdale excavated at Shag River (1915 onwards), Tahakopa (1937-8), and Waitaki (1939), while Skinner reviewed the earlier finds at Moa-bone Point Cave, and Monck's Cave, Sumner. Unfortunately these admirably conducted and accurately recorded excavations disclosed no burial finds, stratification could not be established, and the artifacts found were not distinctive enough to warrant any but the negative conclusion that the material culture of the Otago-Southland Moa-hunter Maoris was not Melanesian, with the further implication that it differed in no way from that of the tribes of the late 18th century.

The wealth of grave offerings from the twenty-nine burials found at the Wairau site (1939 onwards) has resolved this riddle of likeness versus difference by revealing a material culture sufficiently like 18th century Maori culture to be regarded as the production of a people essentially similar to the Fleet Maoris, but different enough to be regarded as ancestral and originating in pre-Fleet times (Duff, 1942). I should say more properly that this is my interpretation of the evidence. Putting this hypothesis in another way; the material culture of the Maoris as Cook found them differed so strongly from that of the nearest islands on any possible migration route from Polynesia that it is difficult at times to see any connexion. From an isolation of at least five centuries in a new and unique environment, Maori culture had diverged sharply, sometimes almost unrecognizably, from its Polynesian prototype. The Maori collections in museums normally represent the end product of this divergence.

The Wairau culture on the other hand may be taken to represent a stage when the divergence of New Zealand material culture from the presumed Hawaiki culture was not so great that its Eastern Polynesian affiliations cannot be recognized, while sharing enough common features with recent Maori culture to be regarded as a stage in the evolution of the latter.

The trend of archaeological researches to date thus renders the hypothesis of a non-Polynesian migration increasingly more difficult to sustain, without quite quashing it.

LITERATURE CITED

- Best, Elsdon, 1916. 'Maori and Maruiwi'. *Transactions of the New Zealand Institute*, vol. 48 pp. 435-47.
Colenso, W., 1843. 'The moa'. *Tasmanian Journal of Natural Science*, vol. 1.
Duff, R. S., 1942. 'Moa-hunters of the Wairau'. *Records of Canterbury Museum (N.Z.)*, vol. 5, no. 1, pp. 1-43.
Falla, R. A., 1942. 'Bird remains from Moa-hunter camps'. *Records of Canterbury Museum (N.Z.)*, vol. 5, no. 1, pp. 43-9.
Forbes, H. O., 1893. 'The Chatham Islands'. *Fortnightly Review*, May, p. 669.

MOAS AND MAN

- Haast, Julius von, 1870. 'Letter to Richard Owen on Rakaia middens'. *Proceedings of the Zoological Society of London*, no. 4, pp. 53-7.
- , 1872. 'Moas and Moa Hunters'. *Transactions of the New Zealand Institute*, vol. 4, p. 93.
- Skinner, H. D., 1923. 'Archaeology of Canterbury (1): Moa-bone Point Cave'. *Records of Canterbury Museum (N.Z.)*, vol. 2, no. 3, pp. 93-104.
- , 1924. 'Archaeology of Canterbury (2): Monck's Cave'. *Records of Canterbury Museum (N.Z.)*, vol. 2, no. 4, pp. 151-62.
- Smith, S. Percy, 1915. The lore of the Whare-wananga, or teachings of Maori college, pt. 2, Te Kauwae-raro or 'things terrestrial'. *Polynesian Society, Memoirs*, vol. 4.
- Teviotdale, D., 1924. 'Excavations near the mouth of the Shag River'. *Journal of the Polynesian Society*, vol. 33, pp. 3-10.
- , 1922. 'Material culture of the Moa-hunters in Murihiku'. *Journal of the Polynesian Society*, vol. 41, pp. 81-120.
- Williams, H. W., 1937. 'The Maruiwi myth'. *Journal of the Polynesian Society*, vol. 46, pp. 105-22.

The Open Field in Devonshire

by H. P. R. FINBERG

IT has been generally agreed that Devonshire lies outside the area formerly cultivated under the open-field system. The map which serves as frontispiece to Gray's monograph on the subject shows the western boundary of the open-field area beginning in west Dorsetshire and passing up northward across Somerset so as to exclude Devon, Cornwall, and west Somerset (1). Dr and Mrs Orwin, while revising and correcting Gray's data at several points, are emphatic where the south-western counties are concerned. 'In *Lancashire, Devon, and Cornwall*, there is nothing to indicate that the system [of open fields] was ever followed (2)'. Recent text-books naturally follow in the wake of these authorities. Professor Darby, for example, writes that in Cornwall, and by implication in Devon also, the prevailing type of rural economy 'had no relation to the three-field system'; and he illustrates his remarks with a reproduction of Gray's map (3).

One well-known fact, which at first sight appears irreconcilable with these pronouncements, was not overlooked by the authors. I refer to the existence at Braunton, in north-west Devon, of an open field of some 350 acres, divided into nearly five hundred arable strips of intermixed ownership. 'Some persons own very many of the strips scattered all over the field; that is to say, several strips in almost every division of it. Others have a few only, one here and there. But in all cases the strips of one owner are everywhere separated from each other by interposed strips of other owners . . . The line of demarcation between any two strips is commonly indicated by a narrow unploughed balk . . . The lesser plots appear as a rule to approximate in area to half an acre, more or less, and the others to multiples of this quantity . . . Very few exceed the limit of two acres' (4).

Concerning this field Gray propounds a theory which seems designed to explain it away. According to him, 'Its position on the map and its low-lying character suggest that it is land at some time reclaimed from the marshes; the two other manors in Braunton not adjacent to the marshes have no open field . . . The extensive scattering of the strips may have been due to the gradual reclamation of the area, each furlong having been subdivided by lord and freeholders as it was improved . . . If these conjectures be correct, Braunton Great Field was of relatively recent origin' (5). The Orwins accept this conclusion, and add some conjectures of their own. 'The Great Field is probably a reclamation, comparatively recent, from the river estuary, rather than a survival of ancient cultivation. The tenants occupy their holdings in it in conjunction with enclosed fields on the higher land adjacent, and probably the Great Field was once common grazing, which was allotted in blocks to those who had grazing rights in it

¹ Gray, *English Field Systems*, 1915, frontispiece and p. 63.

² Orwin, *The Open Fields*, 1938, p. 61.

³ *An Historical Geography of England before A.D. 1800*, ed. Darby, 1948, pp. 194, 207.

⁴ Phear, 'Notes on Braunton Great Field' (*Transactions of the Devonshire Association*, xxi, 1889, p. 202). Venn, *The Foundations of Agricultural Economics*, 1923, p. 14, gives an excellent though brief account of the Field, illustrated by two photographs in which the grass balks are plainly visible (plate ix).

⁵ Gray, *op. cit.*, pp. 262, 263.

THE OPEN FIELD IN DEVONSHIRE

when the silting of the estuary had reached a level at which the grazing of this rich soil could give place to cultivation' (6).

In the course of this paper it will appear that open fields were at one time by no means rare in Devonshire. But before examining the data from other parts of the county, it will be advisable to clear up once for all the doubts which have been expressed concerning Branton Great Field. The evidence has been accessible in print for the last fifty years, and is incontrovertible. An entry in the Calendar of Close Rolls shows that on the 20th of March, 1324, the escheator of Devon made an assignment of dower to Eleanor, widow of Ralph de Gorges, lord of the manor of Branton Gorges. The widow received, *inter alia*, 26½ acres of the demesne arable, made up as follows :

1 a.	in the <i>cultura</i>	Underfayrlinch, in two parcels;
1 a.	„ „	at Schorteland ;
1 a.	„ „	at Le Aliene ;
2 a.	„ „	of Myddelforlong, in two parcels ;
3 a.	in four parcels on	La Merlane [<i>sic</i> ; cf. next item] ;
3 a.	„ „	in the <i>cultura</i> under La Morlane ;
1 a.	in the <i>cultura</i>	Bysoutheye ;
1 a.	„ „	Bywestegreneweye ;
1 a.	„ „	Bystegreneweye ;
1 a.	„ „	Bywestestriclane ;
2 a.	the most southern in the	<i>cultura</i> at La Putte ;
2 a.	in the <i>cultura</i>	Bystecharthurn [<i>sic</i> ; cf. next item] ;
2 a.	„ „	Bywestelathurn ;
1 a.	„ „	at La Crofta ;
1 a.	„ „	on Smerham ;
½ a.	„ „	at La Longeland ;
1 a.	„ „	Bystesmaleweye ;
1 a.	„ „	at La Cok ;
1 a.	„ „	called Stonacre ;
26½ a.	(7)	

Here is documentary proof that dispersed arable strips were to be found at Branton six hundred years ago and more. Further, some at least of these strips lay within the area of the present Great Field. The field is divided into a number of shots or furlongs, each bearing a distinctive name ; and several of the names are recognizable in the document just quoted. 'La Putte' is clearly Pitlands ; 'La Longeland' is the present Longlands ; 'La Crofta' is now divided into Higher and Lower Croftner. The 'greneweye' can be identified from the tithe map as Greenaway Lane, being the southern portion of what is now called Second Field Lane. The *culturae* east and west of the thorn-tree ('By-este-la-thurn' and 'By-weste-la-thurn') lie today in Higher, Middle, and Lower Thorn.

At this point a glance at the six-inch Ordnance Survey map will be found instructive (8). Although surrounded by outlying hamlets, Branton itself presents

¹ Orwin, *op. cit.*, p. 46.

² *Cal. Close Rolls*, 1323-7, p. 333.

³ Part of it is reproduced in *The Land of Britain : Report of the Land Utilisation Survey*, ed. Stamp, Part 92, Devonshire, p. 501, fig. 21.

ANTIQUITY

the appearance of a typical 'nucleated' village. On its western side, immediately to the north of the Great Field, are fifty or more small enclosed fields, which, by their minute acreage and oblong shapes, irresistibly suggest that they are fossilized remnants of strip cultivation. A number of other fields presenting the same appearance are to be found adjoining the Great Field on the west and south. It is in these enclosures that we must locate some of the strips named in the allotment of 1324. The acre 'under Fairlinch' lay at the foot of the slope which bears that name, due west of the village. Further south, the six acres by and under Moor Lane ('La Morlane') now lie outside the Great Field. So also do two oblong fields adjoining Pitlands on the south, which are almost certainly identifiable with 'the two southernmost acres in the *cultura* at La Putte'.

Passing to the east of the village, we enter the manor of Braunton Abbots. Here, between the village and Park Farm, we observe another group of strips, once open but long since enclosed. The 6-inch map reveals their original character; but discoveries to which the map affords no clue are to be made by climbing Watery Lane. On the left, around the steeply sloping flank of the Combes, are unfenced strips, of late years cultivated as allotments; and on reaching the top of the hill one comes upon a new series of arable strips and grass balks, a repetition of the Great Field on a smaller scale. Up here on Braunton Down, three hundred and fifty feet above sea level, open-field cultivation is being carried on today as it has been without a break from time immemorial. There can be little doubt that the manor of Braunton Abbots has been superimposed upon the former east field, and that of Braunton Gorges upon the west field, of the primitive Braunton field system (9). Further north, at Boode, Halsinger, and Winsham, three Braunton hamlets, and still more distinctly at North Buckland in the parish of Georgeham, the present configuration of the enclosed fields indicates former strip cultivation. All of these places are four hundred feet above sea level.

Nothing, therefore, could be further from the truth than Gray's assertion that only one of the Braunton manors contains any open field. As for the 'relatively recent origin' of the Great Field, we have seen that it is over six hundred years old; and so far from being the result of gradual 'reclamation' from the estuary, it is but the shrunken remnant of an open-field system which at one time reached up to, and still partly occupies, the highest grounds in the parish. Furthermore, the six-inch map reveals that not a few of the neighbouring villages and hamlets formerly had similar field systems of their own. The real problem that awaits solution—and it is one which may be recommended as a subject for detailed local research—is why the process of enclosure has been so much less completely carried out at Braunton than elsewhere (10).

Among the surviving muniments of Tavistock Abbey is a charter whereby Richard de Ocbear, lord of the small manor, or sub-manor, of Ogbear in the parish of Tavistock,

⁹ If so, we should expect to find some tenants holding lands of equal extent in both manors. As a matter of fact, some did. For example, in 1502 John Fortescue of Weare Giffard held 50 a. in Braunton Abbots, valued at 20s, by fealty and a rent of 3s 2d, and 50 a. in Braunton Gorges, also valued at 20s, by fealty and a rent of 2s 4d (*Calendar of Inquisitions post mortem*, 2nd Ser., II, p. 390).

¹⁰ The custom of the manor here has been too strong even for the Crown, as appeared some ten years ago, when the Air Ministry proposed to turn the Great Field into an aerodrome. The plan had to be dropped 'because the land is shared by some fifty or sixty owners whose farms are on the hills to the north, and the transactions would have been too complicated, apart from throwing out of gear some fifty established farms, the value of which (at an average rental of £1 an acre) depended very largely on the possession of a portion of the field'. (*The Land of Britain*, p. 515).

THE OPEN FIELD IN DEVONSHIRE

conveyed to Walter his son, in return for a payment of 100s and a yearly rent of 6*d*, a close, a curtilage, and two ferlings of land in Ogbear, made up as follows :

4 a. between the river Lumburn and Ogbear ;
10 a. in the furlong between la Torre and Ogbear ;
11 a. in Yerkysburghe furlong ;
2½ a. in Broken-Cross furlong ;
2½ a. west of Broken-Cross furlong ;

30 a., and also 1a. of meadow.

Here again we have the unmistakable language of open-field cultivation. The charter is undated, but the names of the witnesses date it *c.* 1302 (11). It is noticeable that two thirds of this arable lay in blocks within two furlongs, but whether the strips were contiguous or scattered is not clear. Ogbear is mentioned, though not by name, in Domesday Book, as one of the estates on the fringe of Tavistock which had been held as dependent thegn-lands under the abbot until the Norman conquest. These lands were afterwards used to endow the abbot's Norman knights (12).

The account rendered at Michaelmas 1393 by the reeve of Denbury, another manor belonging to the abbot of Tavistock, shows that whereas barley was then cultivated on about seven acres of seemingly enclosed ground described as 'the lord's land at Hewelegh', wheat was being sown on sixteen acres of 'the lord's land in divers furlongs in the Combes' (*in diversis culturis in le Combes*). This is exactly the phrase that would have been used in reference to the still existing unfenced strips in the Combes at Braunton. Another fifteen acres of the demesne arable 'in divers furlongs' were given up to oats. It is clear that Denbury must be added to the list of manors formerly cultivated under the open-field system (13). The same phrase is applied in 1326 to 862 a. of demesne arable at Kingsdon, 536 a. at Tawstock, and 189 a. at Bovey Tracy ; but in the last-named manor enclosure had already begun, for there were in addition twenty acres not thus described, and they were valued at 6*d* an acre, twice as much as those in the open field (14).

The cartulary of Canonsleigh, now one of the Harleian MSS in the British Museum, contains a number of manorial extents drawn up in 1323. From this source we learn that the demesne arable at Netherton in Farway consisted of

45 a. in Estfeld ;
43 a. in Myddelfeld cum Chelshamcrofte ;
45 a. in Westfeld.

133 a., valued at 3*d* an acre (15).

¹¹ Wcburn muniments, D Bdle 41, no. 2.

¹² I have dealt more fully with the subject of open-field arable and enclosure at Tavistock in a forthcoming work on *The Estates of Tavistock Abbey*.

¹³ Wcburn muniments, G Bdle 5, no. 3.

¹⁴ P.F.O. Inq. post mortem, C134, File 99. Twenty-seven years later the demesne arable at Bovey Tracy was reported to consist of 24 a., worth 10s ; 37 a. in the east of the manor, also 10s ; 70 a. in the north, 40 a. in the west, and 25 a. in the field called Heathfield, all valued at 2*d* an acre (*ibid.*, Miscellaneous Inquisitions, C145, File 169, no. 4).

¹⁵ Harl. Ms. 3660, fo. 178.

ANTIQUITY

Netherton was not a Domesday manor. Northleigh, an independent estate before the Conquest, had been absorbed into the great Mortain fief, and subsequently partitioned (16). The abbess of Canonsleigh held one half of the manor, and her arable consisted of

11½ a. in Campo Australi juxta Wytemor ;
15 a. in Middelfeld ;
14 a. in Northfeld.

—
40½ a., valued at 2*d* an acre (17).

Both at Netherton and Northleigh we seem to be in presence of a three-field system. By contrast with these manors, the demesne arable at Canonsleigh itself appears to have lain partly in the furlongs of the open field, or what had been such until recently, and partly in enclosures. It is described as follows :

18½ a. in forlang' juxta grangiam Curie versus Occidentem in Overeforlang ;
19 a. 1 r. in Nythereforlang ;
18½ a. in Pylelonde ;
19 a. 1 r. in Eldemarnelonde ;
12½ a. in Haybeare ;
9 a. in Nyenakerlond ;
12 a. in Pugeham juxta terram Pouke de Geffreyeshulle ;
22½ a. in Knollelond ;
28½ a. in Seleham ;
2½ a. 1 r. in Langemedede ;
12 a. in Les byrches ;
8½ a. in La Legh ;
4½ a. in una cultura super la Knolle ;
3½ a. in alia cultura super la Knolle juxta volatile Wodecoccorum ;
3½ a. in Wollelegh ;
50½ a. in tota terra Byestebrok.

—
245 a. 1 r., valued at an average of 4*d* an acre (18).

No ploughs at all had been at work on this manor in 1086, although it was then described as having land for one plough (19). At Rockbeare the abbess had 202 a. of arable in sixteen parcels, all apparently enclosed (20).

Two brief references may be quoted from the unpublished cartularies of Otterton and Newenham. In the first, there is record of an exchange of certain acres 'in *parva furlanga de Churlebroke*' in the manor of Yarcombe (21). In the second there are references to land 'in Axminster field' on the west of that township, and to scattered acres, some of them in Cleyfurlong, others in 'the furlong above Rudmede' (22).

¹⁶ DB IV, p. 197 (Lega) ; Reichel, *The Hundreds of Devon*, p. 359.

¹⁷ Harl. MS. 3660, fo. 176.

¹⁸ Ibid., fo. 167.

¹⁹ DB IV, p. 368 (Leiga).

²⁰ Harl. MS. 3660, fo. 143.

²¹ Oliver, *Monasticon Dioecesis Exoniensis*, 1846, p. 258, no. 52. The fifteenth-century ministers' accounts of Yarcombe refer to a demesne close named Furlong.

²² B.M. Add. MS. 28649 (Prince's copy of Sir W. Pole's extracts from the Newenham cartulary and other documents), ff. 424, 428, 433.

THE OPEN FIELD IN DEVONSHIRE

In 1324 the demesne arable at Ermington consisted of 82 a. in severalty, valued at 6d an acre, and 36 a. *in communi*, valued at 2d (23). At Woodbury it consisted, in the thirteenth century, of 100 a. valued at 1d each and 80 a. taken in from the waste. These last were valued at 2d an acre in 1288, but by 1321 they had gone back to waste. There remained the original hundred acres, which evidently lay in open field, for in 1362 they were described as 'one ploughland, worth nothing when unsown, because the land lies in common' (*que nichil valet quando jacet frisca, quia terra jacet in communi*) (24). In the same year a hundred acres of demesne arable at Stoke Fleming were valued at 2d each when sown, and nothing when unsown, for the same reason (25). A similar description of the arable at Loddiswell (80 a.), Ideford (40 a.), and Battishorn in Honiton (80 a.) suggests a three-field system, for in each case we are told that two thirds of the acreage can be sown, while the other third lies fallow and in common (*jacet ad warecturam et in communi*) (26).

In 1334 Sir Henry de Pomeray granted a lease of a house in Brixham 'and land there in Bretele Furlong' (27). Gray overlooked this document, but he found a reference to '*communes campi*' at Brixham in 1523, which he correctly interpreted as meaning open arable field (28). He also quotes a survey of Woodhuish, in the same parish, drawn up in 1566, when the arable of that township consisted of 652 a. lying for the most part 'in twoe common felde'. The holdings were rated at so many ferlings, to each of which were assigned, upon an average, 27 a. of 'arable land lying at large in the fields and *lez Breches*' (29).

The eighteenth century estate maps of Clifton and Clifton Gerrard, in the parish of Broad Cyst, exhibit all the signs of fossilized strip cultivation that have been noticed outside the Great Field at Braunton. Round each of these hamlets is a patchwork of intermixed arable strips enclosed by hedges (30). The same features are displayed even more clearly on an estate map of St. Marychurch drawn up c. 1775. Most of the strips at St. Marychurch had been enclosed, but some of them were still divided only by turf walks, locally termed *landscors*; and any one who held land so divided was said to hold *on* or *by landscors* (31). The same expression was used at Woodhuish and Ilminster to denote intermixture of holdings (32). A large-scale map, drawn in 1820, of Mollard, an extremely hilly parish on the southern edge of Exmoor, shows groups of fields divided into strips, one such group being named, significantly, *New-parks*. They are dotted about the parish, forming islands of strip cultivation in a district already for the most part enclosed (33).

²³ P.L.O. Inq. post mortem, C134, File 81 (3).

²⁴ *Ibid.*, C133, File 54 (4); C134, File 66 (21); C135, File 168 (3).

²⁵ *Ibid.*, C135, File 169 (4).

²⁶ *Ibid.*, C135, File 155 (12). The sown portion was valued at 1d an acre at Loddiswell and Ideford, 2d at Battishorn; and the fallow at *nil*.

²⁷ Historical MSS Commission, 15th Report, Appendix VII, p. 137.

²⁸ Gray, *op. cit.*, p. 259.

²⁹ *Ibid.*, p. 261. Dr W. G. Hoskins, to whom I am indebted for several references, informs me that the 'two great open common fields' at Sheepwash, referred to in his article on 'The Reclamation of the Waste in Devon' (*Economic History Review*, XIII, 1943, p. 81), were common pastures.

³⁰ *The Land of Britain*, p. 501.

³¹ *Transactions of the Devonshire Association*, XVIII, 1886, pp. 434, 440.

³² Gray, *op. cit.*, p. 261.

³³ *Ex inf.* Dr W. G. Hoskins.

ANTIQUITY

Finally, the authors of *The Place-Names of Devon* have noted several field-names, as for example Hedlonde and Forelond, which they no doubt correctly take to be associated with strips in common fields. They mention also two or three compounds of *furlong* (34). Unfortunately the scale of their work has precluded them from stating where these names occur. But enough has been said to demonstrate that open arable fields at one time existed in every part of Devon. Clearly it will not be possible in future to rule out all connexion between Devonshire and the midland system, or to account for every one of its distinctive agrarian practices as the result of 'Celtic influence'.

On the other hand, there is plenty of converging evidence to prove that Devon was a county in which the process of enclosure began early, and in which the isolated farmstead had a place from the first. On these points the received opinion is not likely to be upset. But a systematic investigation of the whole subject is now urgently required. In particular, it is to be hoped that some one will be moved to undertake a detailed study of the remarkable survival at Braunton, tracing the history of the Great Field from the remote past and providing a full description of its recent and present management. The Great Field may well be a thousand years old, and certainly merits as painstaking an investigation as has been devoted to the surviving open fields at Laxton and elsewhere. Aerial photography, and a careful scrutiny of the six-inch Ordnance maps, combined with research into the extant documents of the thirteenth and early fourteenth centuries, will doubtless bring to light traces of many other formerly open fields in Devonshire. When, thanks to such investigation, we have acquired a fuller understanding of the morphology of Devon villages and hamlets, some retrospective light may be thrown upon that most obscure phase of Old English history, the Anglo-Saxon occupation of Devon. The contrast between the 'nucleated' countryside and the land of dispersed settlement may then appear less dramatic than it did when Maitland first called attention to it; but we shall perhaps have more reason to admire the adaptability with which the early English colonists varied their agricultural technique to suit the requirements of this difficult new territory.

POSTSCRIPT

Since writing this paper I have had the opportunity of visiting Braunton Great Field and Braunton Down under the guidance of a well-known local resident, Mr A. H. Slee. The first thing that strikes one is the total absence of those alternating ridges and furrows believed by some writers to have been essential characteristics of the open field. The perfectly flat surface of the Great Field is broken only by the grass balks, which here as elsewhere in Devon are called *landscores* (or, in the local pronunciation, *landsheds*). These turf boundaries, most of them little more than a foot wide, are more economical of space than any hedge-bank, and do not harbour so many rabbits, but on the other hand they propagate couch-grass and are infested by countless rats. In 1889 there were 491 strips, or 'lands', as they are called locally, divided among fifty-six proprietors. Today the proprietors number only twenty, two of them holding each a single land. Where the larger owners have succeeded in consolidating their lands the intermediate *landscores* have disappeared; but those which remain are numerous enough to show quite clearly the original size and shape of the strips. In 1843 Higher Thorn, the area of which is 20.367 a., contained thirty lands, and Middle Thorn, which

³⁴ Gover, Mawer, and Stenton, *The Place-Names of Devon*, 1931-2, p. 690. In the early years of the thirteenth century there is mention of land in Moor-furlong ('Morvorlange') at Buckereil (Hist. MSS Comm., Var. Coll., iv, p. 59).

THE OPEN FIELD IN DEVONSHIRE

is 15.92 a., contained nineteen. These have now been reduced to twenty-four and fourteen respectively.

In 1938 the cereal crops raised on the Field were :

Wheat ..	2903 bushels	Oats ..	2380 bushels
Barley ..	2507 „	Dredge ..	1302 „

Potatoes, broccoli, and sugar beet are also grown. Mr Slee informs me that within living memory the Field was thrown open for common grazing for a month or two after Michaelmas, but that the ordinary pastures lay in the belt of marsh-land, over a mile wide, situated between the Great Field and the estuary.

The Branton tithe map indicates that in 1843 there was a tiny patch of strip cultivation at the north-eastern edge of the parish, on the barton land of the manor of Beer Charter. It also reveals the presence at that date of another patch of strips on the sloping bank of Knowl Water, south of Park Wood and the old Barnstaple road. Neither the shape nor the appearance of the two fields in which this ground is now enclosed gives any hint of its former pattern. In this instance every trace of strip cultivation has been obliterated. It follows that while the existence of ancient strips is frequently betrayed by the present form of their enclosures (as for example in the parish of Goodleigh, two miles inland from Barnstaple, where another such group is plainly visible), the absence of the characteristic strip configuration is no proof that strips did not exist there once. Where there were no ridges to obstruct cross-ploughing, the whole field-pattern could be recast without great difficulty. Further, the principle of 'geographical determinism' enunciated by Mr A. L. Rowse (35) in connexion with the open fields of Cornwall can no longer be upheld, for the evidence of Branton proves that open strips were cultivated on steep hillsides as well as on comparatively level ground. As the case now stands, we must face the possibility that nearly every village and hamlet in Devon may once have had some open field.

³⁵ *Tucor Cornwall*, 1941, p. 34.

The Restoration of the Monastery Church of Debra Damo, Ethiopia

by DEREK H. MATTHEWS

I WAS enthusiastically received by the monks of Debra Damo who gave me every help and encouragement in the project of restoring their church, the most ancient surviving in Ethiopia. The scheme was first mooted by Mr D. R. Buxton, who visited the church and found it in a state of collapse (1). The efforts of Mr O. G. S. Crawford, the Society of Antiquaries, and the British Council, resulted in the Emperor's agreeing to spend up to £5,000 on the works, and in my being appointed architect in charge of the restoration. I had the unique opportunity of entering parts of the building not hitherto seen and recorded by Europeans, and during the process of partial demolition I was able to discover the exact method of construction, this knowledge throwing light on ancient Axumite building methods.

It is not known exactly how old the church is. It is certainly the most ancient and perfect of the early Ethiopian churches, and an important example of an early manner of building in that country.

The method of construction is unusual (FIG. 1); the walls consist of tile-like stones set in earth mortar, strengthened with longitudinal beams which are themselves fixed to the walls with cross-pieces, projecting, and called 'monkey-heads' by the Ethiopians. Such a method of construction has been used in countries as far apart as Crete, Asia Minor, and Tibet. It also seems to have been the normal constructional method of the Axumite builders, who translated the pattern of horizontal timber, monkey-head and window framing into the decoration of the storied obelisks of Axum, nearby. This is yet another example of timber forms being translated into stone, a common architectural trend. Even today, the tradition is still alive, and modern church builders in this part of Ethiopia still use timber in a similar way to stabilize their walls.

The preservation of this unique building is undoubtedly due to the fact that the monastery is situated high on a rocky mountain, entirely surrounded by cliffs, and is approachable only by climbing with ropes up the vertical rock face (PLATE IIA). I myself had to scale the cliff on each visit to the monastery, and all building materials (cement-bags, reinforcing rods, sand, stone and scaffolding poles) had to be hauled up the cliff. Its inaccessible position, seven days by lorry from Addis Ababa, and three hours' mule trek across precipitous country from the nearest motor track, made the restoration a difficult, yet fascinating, task.

I was treated with great hospitality by the monks, for word had gone ahead that the Emperor was sending an architect to renew their church. The community gave me an Imperial welcome with their richly coloured robes, umbrellas, and silver crosses, to the accompaniment of chanting, and the music from drums and sistra, and they entertained me with their own brand of beer ('talla'), and excellent honey wine ('tedj'). At all times during the works they were most friendly and helpful, bringing me sheep for meat,

¹ Buxton 1 and 2 (see pp. 196, 198, below).

RESTORATION OF THE MONASTERY CHURCH OF DEBRA DAMO

and eggs and chickens in abundance. At the end of the works they thanked me, saying they wished to give me a token of appreciation, but as they were poor, would I accept thirty Haile Selassie shillings.

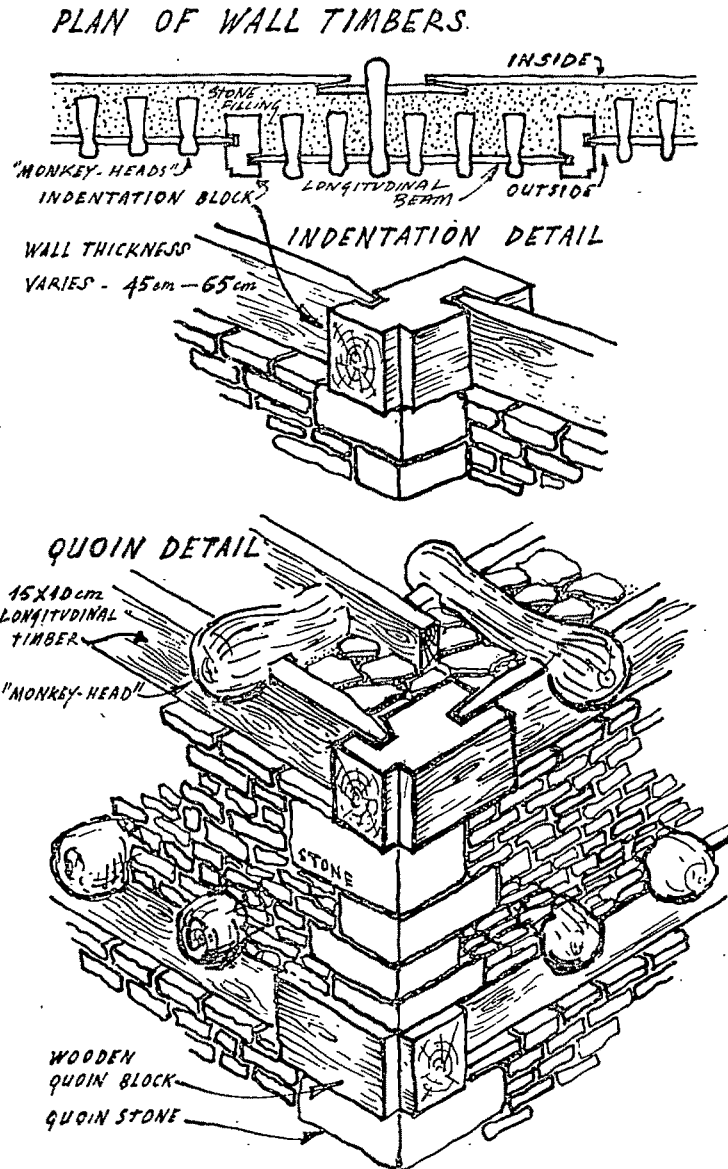


FIG. 1. WALL CONSTRUCTION, SHOWING ARRANGEMENT OF LONGITUDINAL TIMBERS, 'MONKEY HEADS' AND QUOIN BLOCKS

During the course of the works, I covered 5,700 miles in Ethiopia and Eritrea by motor lorry, collecting the necessary workmen and supplies. The difficulty in obtaining petrol for transport further aggravated the initial difficulties. Whilst stone was being

ANTIQUITY

broken on the site, and sand transported from the river, and hauled up the cliff, I had to hunt for other materials, and then transport cement on mule-back during the period of the rains.

Finally having obtained all the necessary supplies, I established myself permanently on the mountain top, with a staff of servants, Italian and Ethiopian masons and labourers. After eighty-two working days, the restoration work was complete. Although we were obliged to work during the period of seasonal rains, only eleven hours were lost on the site because of bad weather.

GENERAL DESCRIPTION OF THE CHURCH

If the building, which is approximately 20 metres by 9, is divided lengthwise into five parts, the two parts on the west end form a part of the building having an upper floor (FIG. 2). There is similarly an upper floor over the section containing the sanctuary and the two adjoining rooms, and there are long lofts over the aisles, flanking the nave. Over the sanctuary is a small dome of timber, now covered with cloth (FIG. 3 and PLATE

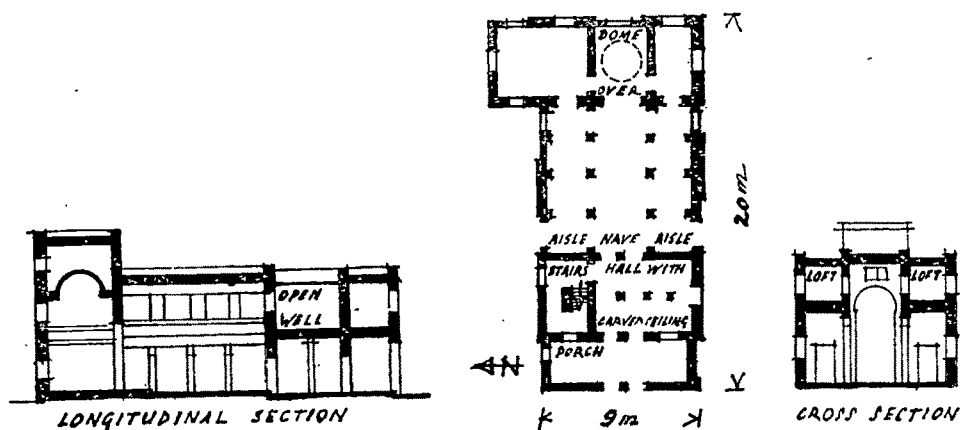


FIG. 2. PLAN AND SECTIONS OF CHURCH

11b). Some painted fragments lying near the dome are apparently infilling panels which formerly were part of the dome. The monks said the paintings were not more than two hundred years old; the only recognizable piece is approximately 40 cm. by 60 cm. and contains two head and shoulder portraits, carrying crosses, and a dove; in Ethiopian characters is written 'Abuna Samuel', 'Abuna Aregawi', and 'Abuna Tacla-Haimanot'. The ribs of the dome are decorated with painted meander patterns. The dome is only two metres in diameter, and is not visible externally, being protected from the weather by a raised portion of the flat earth roof. Three years ago the nave had a fine arched timber roof construction (2), which became unstable and was replaced by the monks themselves with a flat beam and board ceiling, before I arrived.

On the main axis, the entrance leads into two wide vestibules. The first is a partly open porch, of later construction yet similar to the main body of the church; the second, the inner entrance hall, has a fine carved panel ceiling, and is not so wide as the porch, because part of the space is occupied by a staircase which winds round a block of masonry, typically Axumite in construction. This staircase leads to an open light-well

² Buxton 2, Plate 11 d.

RESTORATION OF THE MONASTERY CHURCH OF DEBRA DAMO

over the inner entrance hall, and to the lofts or upper rooms of the church, and the flat earth roofs.

The main part of the church can be entered either from the inner entrance hall by a west door, or by means of doors in the north and south walls. This part of the

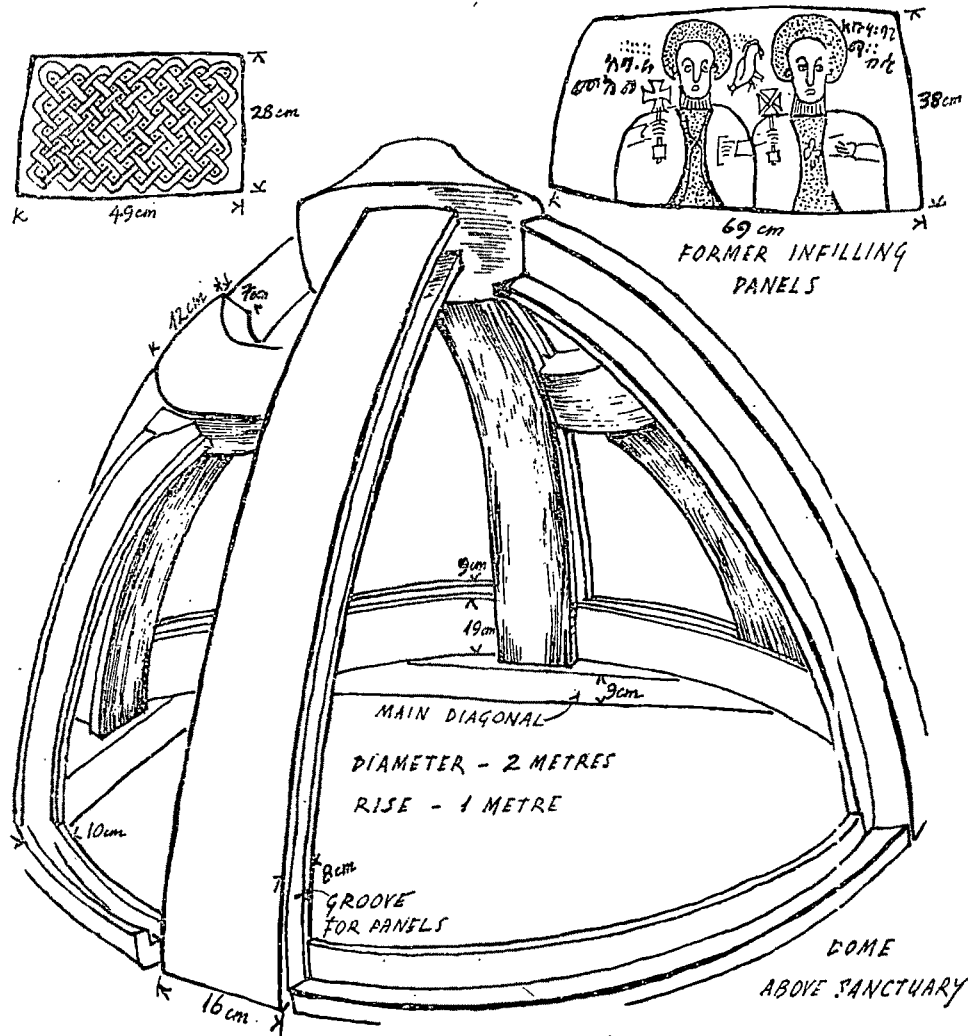


FIG. 3. WOODEN FRAMEWORK OF DOME ABOVE THE SANCTUARY, AND TWO OF THE FORMER INFILLING PANELS
(The dome is now covered with cloth)

church is lighted indirectly by small windows in the lofts above the aisles, and also by ground-floor windows in the aisles themselves, and by a high west window giving on to the light-well.

The nave is separated from the sanctuary by a wall with a carved timber arch (PLATE III). There are doors into the sanctuary from the small rooms on each side of it,

ANTIQUITY

which are in turn reached by doors at the east end of the aisles. The room on the north of the sanctuary is larger than that on the south, and contains the remains of the king

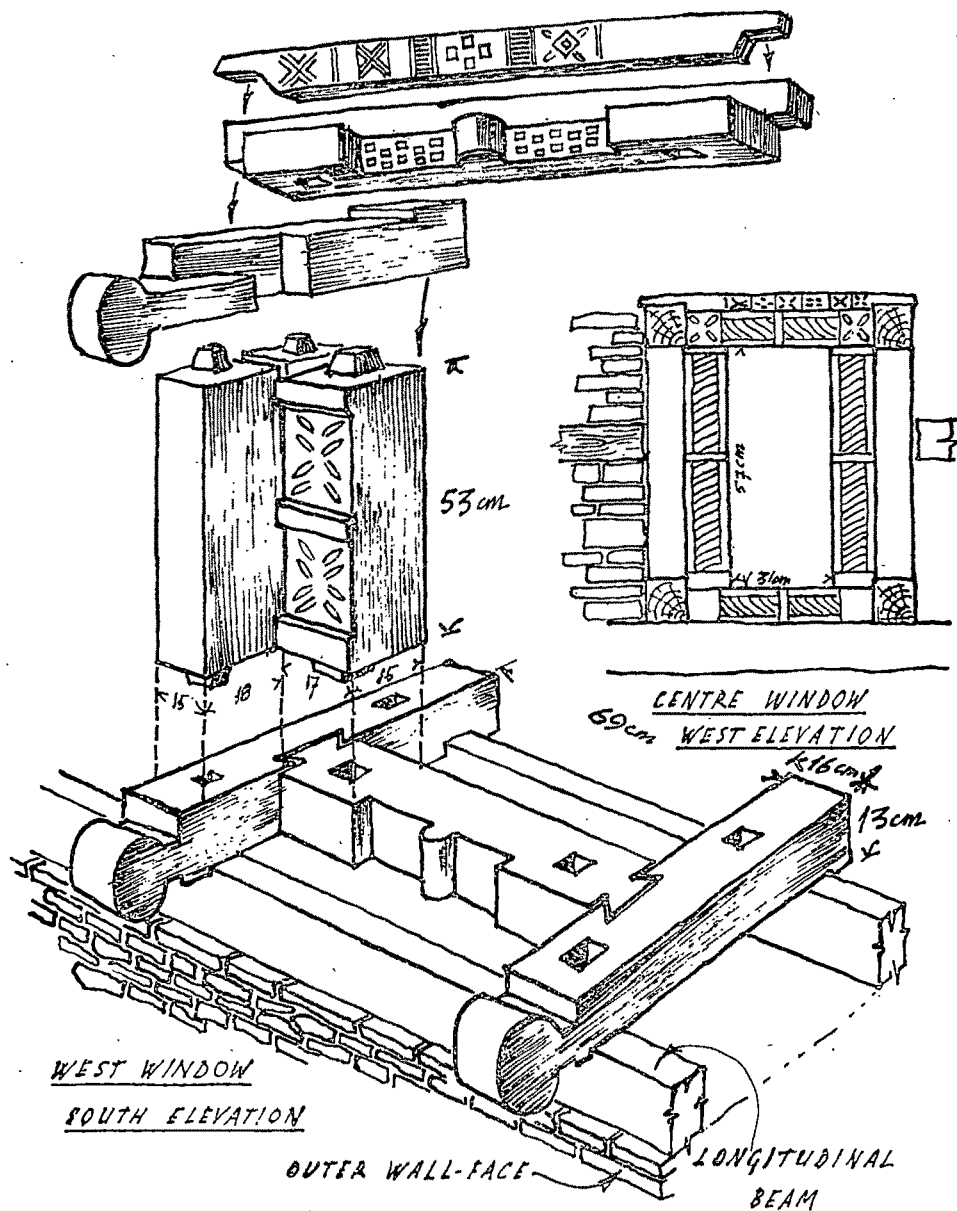


FIG. 4. EXPLODED VIEW OF A WINDOW IN SOUTH WALL SHOWING CONSTRUCTION AND DECORATION, AND ELEVATION OF THE CENTRE WINDOW OF SOUTH ELEVATION

Lebna Dengel, and his son Galadewos ; this room projects from the main body of the building, and for this reason it might have been added at a later date, as it is not part

RESTORATION OF THE MONASTERY CHURCH OF DEBRA DAMO

of the fundamental plan shape. However it is built upon a base which is Axumite in design; this base is only visible on the north of the church, and is stepped in three intervals, like those in the ruins at the Axumite sites of Cohaito, Axum and Jeha. This presupposes a very early remodelling to an original Axumite building, which might date from the pre-Christian Axumite period.

The plan is noteworthy for the unusual indentations within the thickness of the walls. Such a feature is yet another link with old Axumite building methods, for it occurs at the other Axumite sites in Ethiopia, and in a minor form, it may be observed in the plans of the storied obelisks at Axum. The reason for the indentations may well be that the distance between them is a convenient length for the longitudinal beams of the wall, whose ends are always mastered by a wooden quoin-block (an architectural fake making the beams appear thicker than they really are). (FIG. 1).

The walls consist of tile-like stones set in excellent earth mortar, with larger quoins of a different stone. The monks say that the small stones for the church came from Eritrea, where there is said to be still a heap of stone that was quarried for the building, at Zabanbur near Adi Caieh. The walls owe their preservation partly to the timber work which stabilizes the wall structure. The effect is of a series of bands on the wall surface consisting of timbers approximately 15 cm. deep, alternating with bands of stonework of varying depth from 24 cm. to 40 cm. The longitudinal timbers are fixed, and kept upright, by a series of cross-logs notched over them, and built into the wall ('monkey-heads'). A very few (not all) of these cross-logs pass through the wall, and are notched over the twin beam on the inside. The long beams are further tied together by means of the window construction (FIG. 4 and PLATE IVa). The longitudinal timbers of the wall are necessary owing to the small size of the stone used, and the fact that it is set in earth mortar. Olive wood is used for all constructional parts, except in the flat roof construction, where euphorbia is used.

Widely projecting slates form string courses outside at both the ceiling levels. A parapet surmounts the wall, with slits to allow rain water to escape from the flat roof.

The windows and doors have the same construction that was interpreted in solid stone in the storied obelisks at Axum, and there are similarities in detail with the windows and doors in both the built-up and rock-hewn churches in Lasta, and other parts of Ethiopia (FIG. 4 and PLATE IVa) (3). The tradition of making openings in walls in this way is still followed in Northern Ethiopia today.

There are six monolithic columns in the nave, one in the inner entrance hall, and the pair built into the outer west wall. They are not all of the same size, and they might have come from another building.

The nave and sanctuary are encircled by a wooden entablature whose metope panels are carved with swastikas, crosses, and plaited and meander decoration, reminiscent of Byzantine and Coptic art (FIG. 5 and PLATE III). Such a timber entablature has been translated into stone elsewhere in the numerous rock-hewn churches of Ethiopia. The carved timber ceiling in the entrance hall contains, in addition, representations of animals (there are no animals carved in the frieze panels) (4).

³ See Buxton's two articles, *passim*.

⁴ In the Asmara Museum there are similar carved panels which were taken from a demolished church in Asmara, of the same type as Debra Damo. [It is hoped to publish a special article on these panels in a later number. Mr Matthews was able to take excellent photographs of all of them. See also Buxton (1), plate IV. Ed.]

ANTIQUITY

THE INFLUENCE OF RITUAL ON PLANNING

It is interesting to note that the plan of the monastery church is not ritually convenient to the monks. They generally make modifications by hanging curtains in the nave to subdivide it. They asked me to make the entrance porch deeper, because it is not large enough for them to play their drums and sistra. The plan of Debra Damo is unlike the modern churches in Ethiopia, which are generally circular, enclosing a square sanctuary; it is nearer to the plans of Axumite ruins. The Debra Damo plan has also been used in the rock-hewn and built-up churches of Lasta, and other regions.

The church is situated in the usual compound, common to all Tigrai churches, surrounded by a wall approximately three metres high, with an elaborate gate-house having an upper floor, and a free-standing bell-tower. The treasury and other buildings are grouped nearby. There is a rock-hewn cistern north of the church, containing holy water.

THE ORGANIZATION OF THE MONASTERY

The monastery buildings are grouped on top of the amba* like a large village (FIG. 6), and are the normal stone houses (with flat earth-roofs) of Tigrai (PLATE I). Each monk has a house with several rooms, including an upper room for contemplation and prayer, and a walled garden (FIG. 7). The spaces between the groups of houses with their walled gardens form winding lanes leading towards the church, the meeting hall, and the rock-hewn cisterns for water supply.

The community is divided into two parts, the 'First Association' composed of those monks belonging to the district, and owning land in the vicinity, and the 'Second Association' which consists of those who come from afar. The latter share all their worldly goods among themselves, and lead a very frugal life. There are large numbers of small boys who come to Debra Damo to learn Geez and to chant. The monastery is thus a place of study as well as prayer. It is also a favoured burial place, and those who can afford it are buried on the top of the amba in caves.

The loose-knit organization of the community allows decisions to be argued out in the assembly of monks, usually out of doors in the shade of gaily-coloured umbrellas. The Head of the monastery is elected by the whole community for a long period. At present there are about three hundred monks; it is said that at one time there were as many as a thousand.

There is a second church on a ledge below the top of the amba, at the point where Abuna Aregawi is reputed to have vanished. Its plan is rectangular, with an inner sanctuary completely surrounded by a corridor. The walls of the inner part are very old, and are similar in character to those of the main church, with timber and stone technique, and wood carvings in the same Ethiopian-Coptic character. The outer wall of the small church is comparatively new, and similar to the rest of the monastery buildings.

THE AGE OF THE CHURCH

I was told by Getai Gabre Ghiorghis, a monk, that the church was first built in the year 537, which is 1405 years ago by the Ethiopian Calendar; and that it had been renewed but not since the time of Hatsay Lebna Dengel (1508-40), who repaired the south wall, and whose remains are in the church.

* Amba means hill or hill-top and from the use of such came to mean 'hill-fort', just as Debra (with a similar meaning) came to mean 'monastery'. ED.

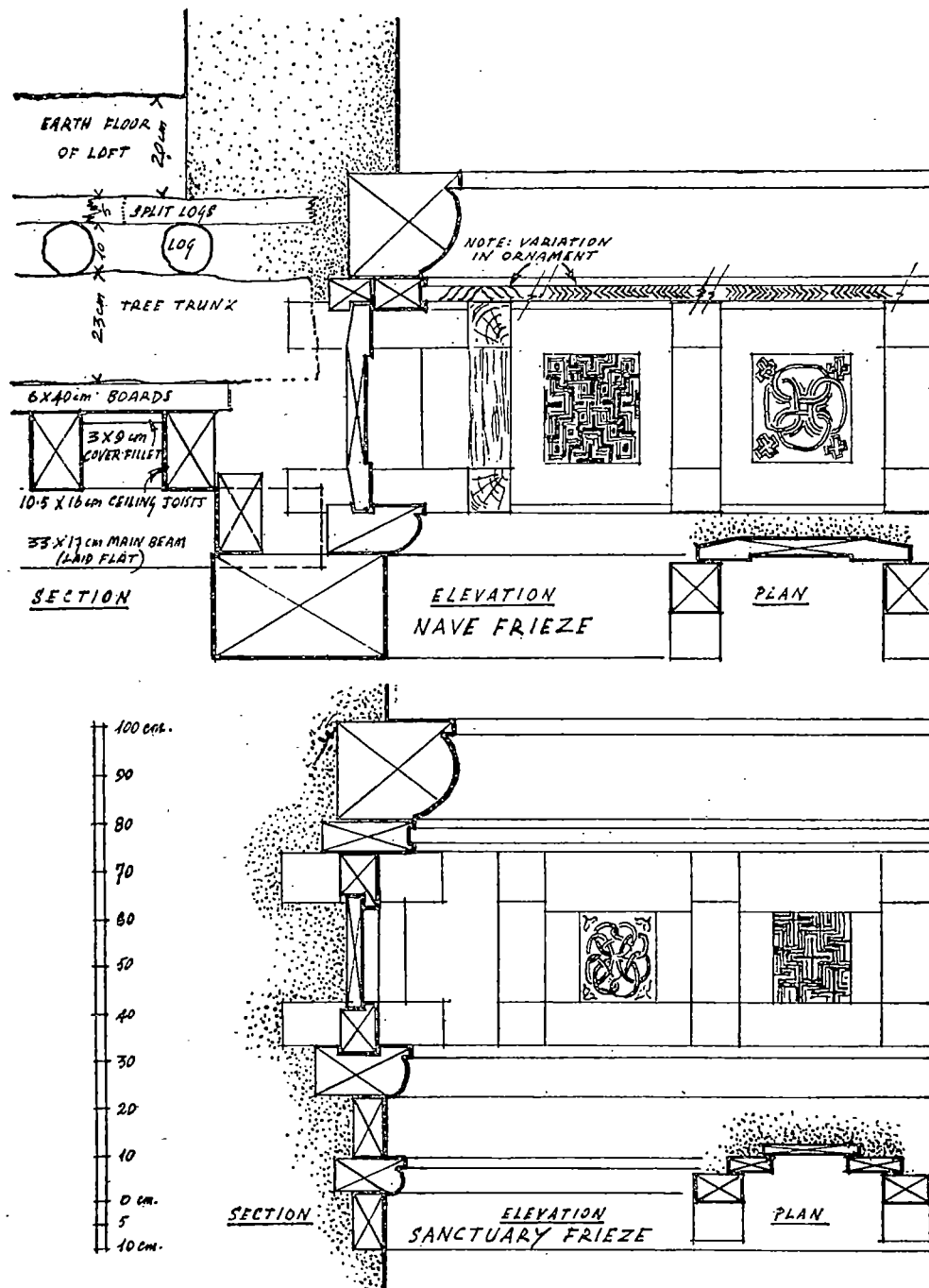


FIG. 5. SECTIONS THROUGH FRIEZES IN NAVE AND SANCTUARY

The panels in nave and sanctuary are different in construction. The section through the nave frieze also illustrates the construction of the floor of the loft above the aisle, which is supported on tree trunks 23 cm. in diameter, this rough carpentry being masked below by boards, ceiling joists and a beam forming a clean finish inside the aisle

ANTIQUITY

Other monks said that the church was built in the year 370 and the plinth of the church (which is Axumite in design) had been built by Hatsay Gabre Maskal; and that Hatsay Lebna Dengel built the upper part.

Regarding the legends of the building, it is difficult to establish whether the king 'Gabre Maskal' is the son of Kaleb (6th century), or the king Amda Tsion (1313-43) whose second name was Gabre Maskal.

The priests said that there had not been a building on the site before the Monastery was founded. They are bound to uphold the legend of the foundation, but it is not unlikely that there was once on this site a pre-Christian Axumite temple or house which was converted into a church when Christianity came; for in addition to the Axumite base, plan shape, and construction, there are three fragments of Axumite capitals lying outside the church, and the monolithic columns now incorporated in the structure are of different sizes, and might have come from an older building. There is also a carved sandstone block built into the gatehouse, looking as if it came from an older building.

From 14th century manuscripts found at Debra Damo by Dr A. Mordini (5) it is apparent that until the 14th century the sister monastery nearby of Debra Libanos (6) was more important, after which Debra Damo came into ascendancy, and was at the height of its power. Debra Damo has been proved to have been a place of pilgrimage and studies at least since the 13th century, and Saint Tacla-Haimanot studied there and received there the garb of a monk. (The many manuscript fragments prove that Debra Damo was active at this time).

The difference between the carved panels in the frieze and those in the ceiling, and the differently cut capitals, make it seem likely that these elements were not all made at the same time, but assembled together in the present form. Indeed some of the ceiling panels look as if they had been cut down to fit the framing. Thus it is not possible to date the building from any one element. The fact that Debra Damo was apparently eclipsed by Debra Libanos, and then acquired a new lease of life in the 14th century, points to the possibility of at least one rebuilding, at that time.

The form and construction of the church relates it directly with early Axumite building, of the period of the storied obelisks. It seems probable that an Axumite building was converted into a church at the foundation of the monastery. It is known that Debra Damo was given a new lease of life after perhaps a period of decline, and would then have been partly rebuilt. This final reconstruction was probably on the same lines as the original Axumite building, and would account for the perplexing survival of such a very ancient style of building.

In my opinion alterations which might have taken place when Debra Damo revived in the 14th century were merely in the form of repairs to the fabric, and not a radical remodelling. In spite of possibly a series of reconstructions, including the one which has only just been completed, the original Axumite character of the building has been retained and the building today is substantially the same as when it was first built (perhaps before the 9th century). It seems unlikely that any alterations were made to the fabric after the death of Lebna Dengel (1540).

THE DEFECTS OF THE BUILDING

I found that irreversible movements had been taking place within the walls. Alternations of rain and sun had set up movements which, over a very long period, had

⁵ Conti Rossini, 'Pergamene di Debra Dammó', *Rivista degli Studi Orientali*, vol. XIX, 1940.

⁶ Not to be confused with the celebrated monastery of Debra Libanos in Southern Ethiopia.

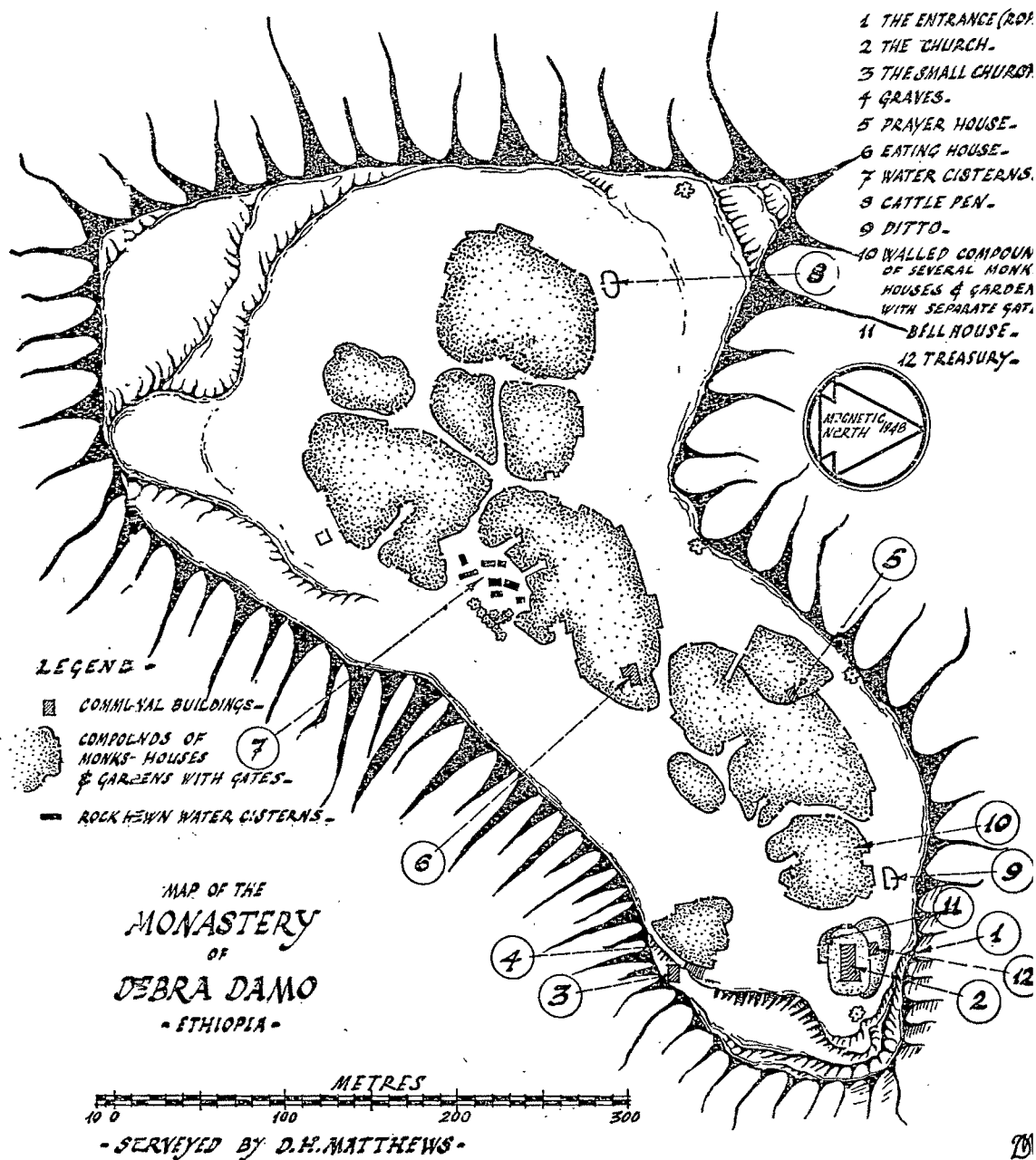


FIG. 6

ANTIQUITY

become noticeable. All the external walls were leaning outwards mushroom-like. The most unstable portions were leaning as much as 45 cm. beyond the vertical, over a height of 6 metres ; the average amount of lean was 25 cm. ; many sections of wall were only 10 cm. out of the vertical.

In many cases the movement had only taken place in the outer half of the wall, with the face of the wall peeling off ; the inner wall planes were not badly affected, except on the north ; there were vertical cracks at the quoins and junctions with cross walls.

The north wall had leaned out so far that it could no longer support the flat earth roof, which had fallen in over the north loft. The building had been shored up some years ago by the Italians, who, as a temporary measure, had covered part of it with corrugated iron sheets. The foundations are sound, being placed upon solid rock.

METHOD OF RESTORATION

The external walls were strengthened by the placing of two reinforced concrete beams, connected together with vertical ties, within the thickness of the walls, encircling the building like belts. In addition, diagonals and cross-ties were placed where necessary. Having completed the work, the strengthening members are not visible from inside or outside the church. The walls which were leaning dangerously were demolished, all timbers indexed, and the whole reassembled in exactly the same form as before. At the same time it was a simple matter to repair minor defects.

The building is now structurally sound and in good order (PLATE IVb).

APPENDIX : LEGENDS OF DEBRA DAMO

THE LEGEND of the Foundation of Debra Damo as related to me at Debra Damo by the Head of the Monastery, Memhir Wolde Mariam :

" Abuna Aregawi was one of the nine Saints who went to Axum. After they had " been there some time, they decided to go out separately to different places to spread " Christianity in Ethiopia. Abuna Aregawi came to the flat-topped mountain where " the monastery now is, and wished he could climb the cliff and live on the top. A " serpent told him that there were only birds and the sun on the top, so Abuna Aregawi " prayed, and the serpent reared itself up and placed the Abuna on the top. Whilst this " was happening the Archangel Gabriel stood by with a sword ready to slay the serpent " if it should attack Abuna Aregawi. When the Abuna went up he dropped his cross, " and it fell on the stone which is today kissed by all who enter or leave the Monastery. " The place where Abuna Aregawi found himself is where the sanctuary is today. The " Abuna then asked Hatsay Gabre Maskal to build a church, which he did ".

TRANSLATION out of ' Gedli Abuna Aregawi ' (Work of Abuna Aregawi), a book in possession of the monk Getai Gabre Giorghis (translated by Ato Haile Zarrafa).

The building of the church of Debra Damo. Gabre Maskal sent his army to collect materials and workmen with knowledge ; he sent to the north, east, south, and west and they brought carts, ladders, and equipment. The carts were three and a half metres long for the stone. He made a ramp in order that the animals and workmen could climb. They brought water and stone from below. He built the church with a lot of wood, fashioned into a good shape. It is very good and takes your heart. He finished the building in two years and built it after he had been king for one year. He collected all the necessary furnishings for the church, and gave them to the Monastery. He gave twelve gold crosses, and gospels in gold and silver, and the story of Paul, and everything

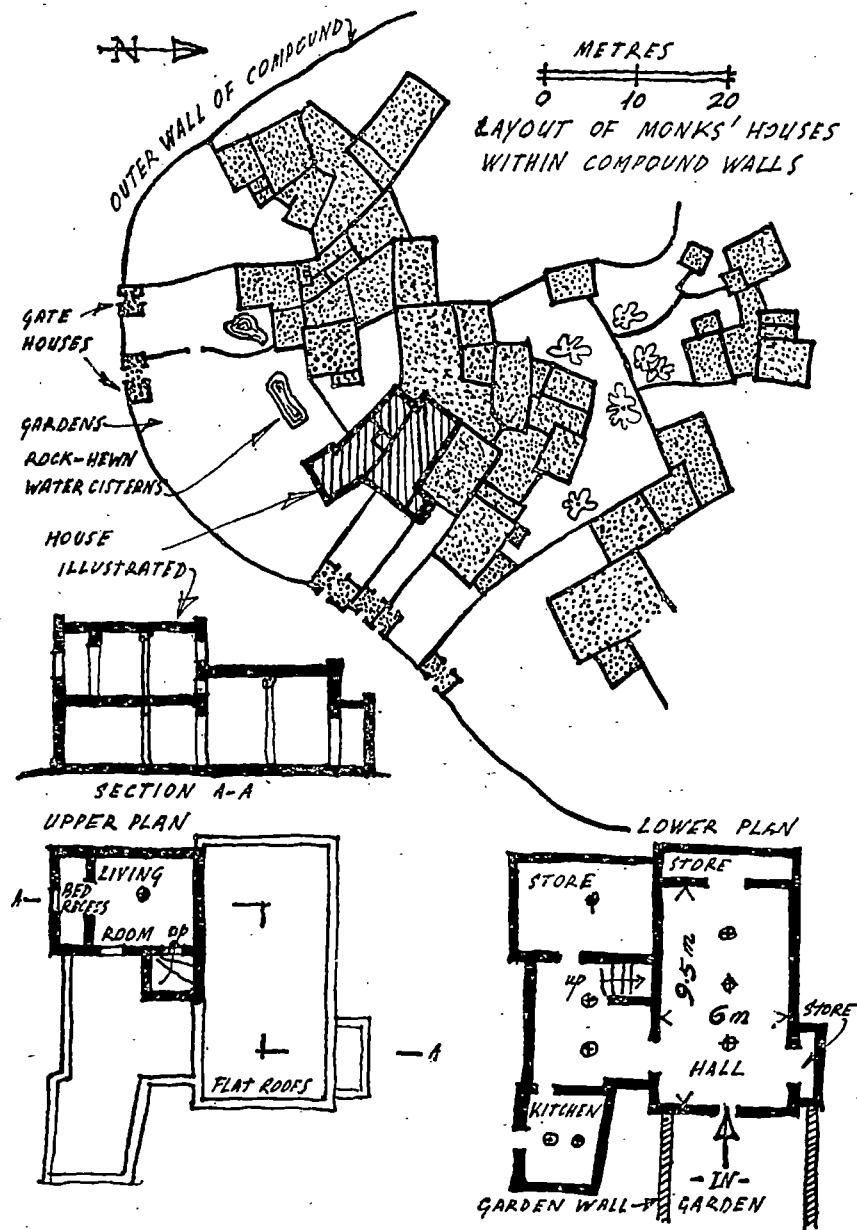


FIG. 7. PLAN OF PART OF ONE OF THE COMPOUNDS, SHEWING THE BACK-TO-BACK HOUSES, WITH INDIVIDUAL GARDENS SURROUNDED BY A COMPOUND WALL WITH GATES. PLANS AND SECTIONS OF A TYPICAL HOUSE

ANTIQUITY

else that is necessary for a church. When Gabre Maskal built churches he started with Debra Damo; it was the second church in Ethiopia after Axum. He gave all his property and riches to Debra Damo. He asked the Abuna Aregawi to become priest, and he agreed and became priest, and they brought angels for him; and the Holy Ghost came over the communion, and over all of them. The Abuna and Gabre Maskal took communion, and also the others; and God spoke with Abuna Aregawi and said that all who took communion that day would be assured of a place in heaven. When he left the church, Gabre Maskal collected all the poor people and also the big people of his government, and made a big feast at Debra Damo, giving food to the hungry and clothes to the naked, for the good of the church, Abuna Aregawi said he would protect Gabre Maskal from danger, and follow him when he died; and Gabre Maskal bowed to the Abuna, and said 'Bless me', and then 'Accompany me', and when he left, he asked whether he should leave the ramp up the cliff, and Abuna Aregawi said 'Dahamemo' which means 'Take it off', and he asked Gabre Maskal to provide something like a serpent to climb with, because it was good that people should feel afraid when they saw the cliff, and pray for God's help; so the ramp was taken away, and a rope made with leather thongs in the form of a serpent as it still exists today. Afterwards the place was called 'Debra Damo', from the words of Abuna Aregawi (Dahamemo). Before this it had been known as 'Debra Halleluya' as it is still sometimes called today, for when Abuna Aregawi had first gone up on the top, he had cried 'Halleluya' to the north, east, south and west.

OTHER LEGENDS on the history of Debra Damo. Abba Haile Mariam, a monk, told me that a pagan queen Gudite, who was burning many churches in Ethiopia, captured Debra Damo and used it as a stronghold for twenty years. At this time, the mountain had been used by the Emperors as a prison where they kept their relatives in captivity to avoid rival claimants to the throne; and at the same time it was a monastery.

When the queen came, some of the monks were killed, and some went away. The queen made a ramp up the cliff, three and a half metres wide, at right angles to the cliff near the present position of the rope, so that animals could go up, and horses and mules were stabled in the church; I was shewn where the threshold of the southernmost of the two western doors had been repaired, after being damaged by the hooves of the animals. During this period, the settlement became an ordinary town, with women living there.

The mountain was eventually retaken from the pagans by a king who brought ladders and scaled the cliff in the night. He brought monks, and re-established the Monastery.

REFERENCES

1. D. R. Buxton. 'Ethiopian rock-hewn churches', *ANTIQUITY* xx, 1946, 60-9.
2. ——. 'The Christian antiquities of Northern Ethiopia', *Archaeologia* xcii, 1947.

PLATE I

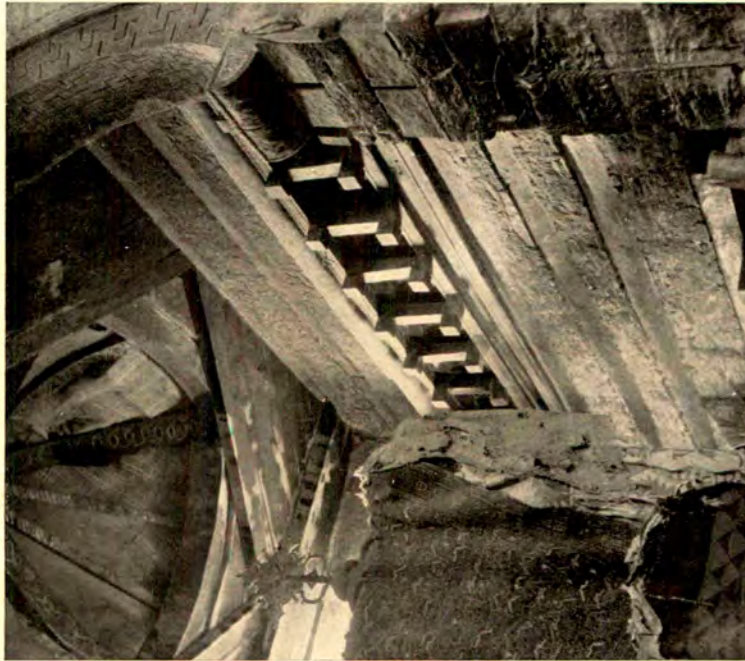


DEBRA DAMO, ETHIOPIA : GENERAL VIEW OF MONASTERY BUILDINGS ON TOP OF THE AMBA (flat topped hill)

PLATE II

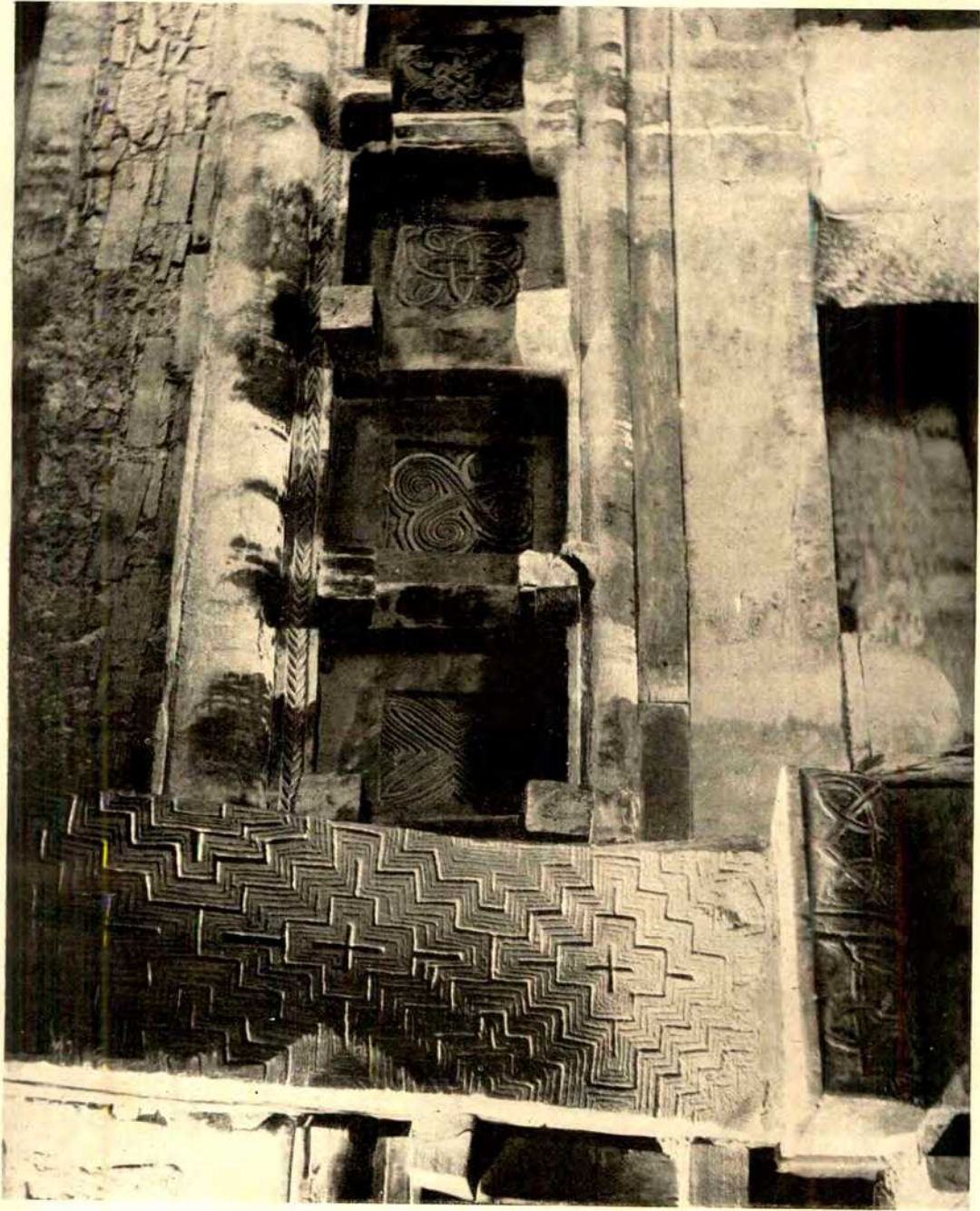


a. DEBRA DAMO : THE ONLY WAY OF APPROACH TO THE MONASTERY
Note bag of cement being hauled up



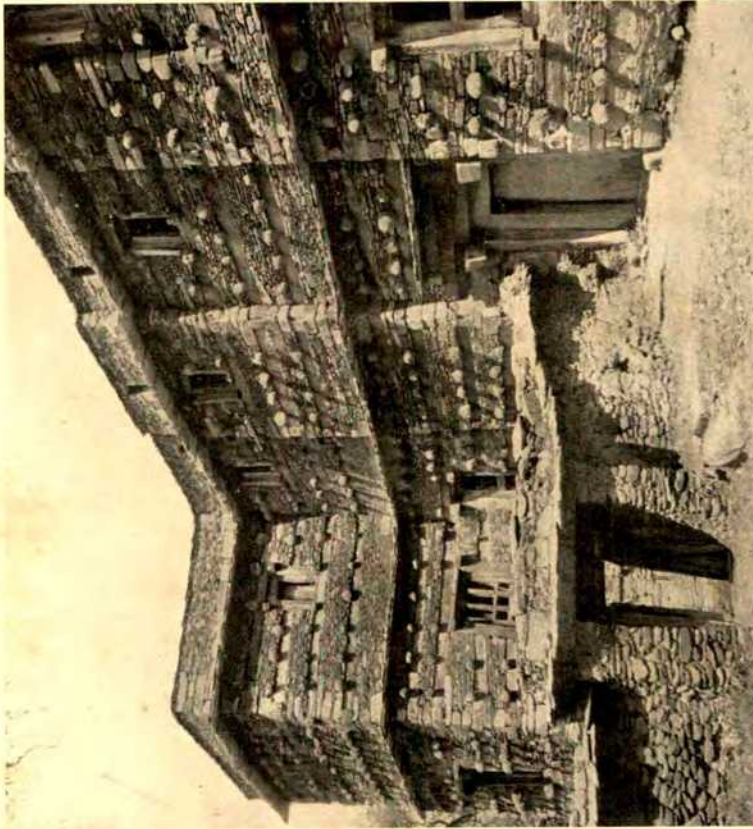
b. DEBRA DAMO: THE DOME OVER THE SANCTUARY

PLATE III



DEBRA DAMO: NAVE FRIEZE AND ARCH BETWEEN NAVE AND SANCTUARY

PLATE IV



b. DEBRA DAMO: STATE OF THE CHURCH AFTER COMPLETION OF WORK



a. DEBRA DAMO: WINDOW SHOWING WOODEN FRAMEWORK
EXPOSED DURING RESTORATION

A second fixed point in the Chronology of the Harappa Culture

by J. F. S. STONE

FIXED points in the chronology of the Harappa Culture are not abundant and such as exist tend rather to emphasize the earlier phases of that remarkable civilization. Evidence for close contact with Akkad about 2300 B.C., based primarily on stamp-seals, has been the subject of review by Piggott (1), and much clearer light has been forthcoming as a result of excavations at Harappa by the Archaeological Survey of India during 1946 under the direction of Professor R. E. M. Wheeler (2). These excavations included cuttings through the rampart of the citadel, and investigations of the later cemeteries superimposed on the culture. Whilst accepting the evidence for the earlier fixed point Wheeler faces up to the problems raised by these cemeteries and is strongly inclined after reviewing the facts available, and certain passages in the Rigveda, to agree with Childe that the 'Cemetery H intruders' may belong to Aryan invaders, the conventional date for whose first incursion into India is the 15th century B.C. As a result he concludes that 'the combined weight, such as it is, of these various indications suggests the millennium 2500-1500 B.C. as a possible inclusive date for the mature Harappa civilization, without prejudice to the still-unplumbed depths of Mohenjo-daro'.

Attention has recently been focussed by Piggott on these later phases of the civilization as a result of a study of the type and distribution of certain spiral headed and animal headed pins, and of a bronze mace-head found at Mohenjo-daro and Chanhu-daro (3). He here sees clear evidence pointing to trade contacts or folk movements from the West and affecting India at the end of the Harappa phase, if not indeed when it was actually defunct, probably 'after 2000 B.C. rather than before and possibly some centuries later'. And with Wheeler he does not appear to be disinclined to accept the traditional date of about 1400 B.C. for the incursions into India; though both are fully aware, and in fact state categorically, that the Akkadian contacts are the only well fixed chronological points.

Now it so happens that another fixed point is available which would appear to fall within and add precision to this debatable latest phase. Beads are easily transportable articles over great distances and as easily lost, and Beck has already drawn attention to a number of virtually identical etched carnelian beads found in Akkadian deposits and at such Indus sites as Mohenjo-daro and Chandu-daro (4). But it is to certain of the Faience bead series that I now wish to draw attention. These are fairly abundant in Harappa Culture contexts, and a number from Harappa itself have been the subject of study by Beck (5), but prior to recourse to spectrographic analysis.

¹ *ANTIQUITY* XVII (1943), 178 ff. and *Ancient India*, no. 1 (1946), 21.

² *Ancient India*, no. 3 (1947), 60.

³ *Ibid*, no. 4 (1948), 26.

⁴ *Antiquaries Journ.*, XIII (1933), 390.

⁵ M. S. Vats, *Excavations at Harappa*, I (1940), 403 ff.

ANTIQUITY

Faience consists almost entirely of finely ground quartz grains cemented together thermally by means of other materials such as an alkali or lime or both together, and subsequently glazed with a coloured, usually blue, glass (6). Such an artificially manufactured material is fairly readily susceptible to analysis, and by modern spectrographic means traces of elements or impurities fortuitously or intentionally introduced can be readily detected. Thus qualitative and quantitative estimation of the main constituents and of traces of impurities permit one to establish identity or dissimilarity when objects from different sources are compared. And it is only natural to expect that the composition of faience must vary considerably from place to place and from time to time in view of its early discovery and long persistence as a manufactured material.

The segmented variety of faience bead is of comparatively common occurrence over great lengths of time, and appears to have been a favourite form in many countries (7). Whilst studying this type of bead in 1935 Beck and I examined two from Harappa (8) and were surprised to find how closely they resembled similar beads found in England. But as they were at that time dated *c.* 2750 B.C. they were discarded as being of too early a date to affect the immediate problem of the origin of the British specimens. These two beads were on string no. 177 of the selected beads submitted for examination and were recorded as having been found at a depth of 19 feet 8 inches in Stratum VII in Square I 12/20 of Mound F (9). Such a method of recording finds in the absence of stratigraphical associations prevents their being used for chronological purposes and, though apparently early, one cannot trust this inference from the published report. All that we need note here is that twenty-three other segmented faience beads were recorded as having been found at different places and in association with various strata (10).

Now very similar segmented beads have been found once again during the recent excavations by Wheeler at Harappa, and two are stated to have come from Period V and post-rampart horizons respectively; whilst of two others, one came from the debris layer and one from a post-Cemetery H layer of the cemetery area (11); all implying a comparatively late date, and certainly not an Akkadian context of *c.* 2750 B.C. Their rarity implies that they were intrusive, and their close similarity to Eastern Mediterranean specimens suggests that some at least were derived from that source. I propose here to adduce evidence to show that the Harappa segmented beads examined by Beck and myself in 1935 were then erroneously dated about 1000 years too early and that in reality *c.* 1600 B.C. would be nearer the mark.

Through the kind assistance of Dr P. D. Ritchie, and of the Courtauld Institute of Art (University of London), a number of specimens of segmented beads from a variety of sources including Harappa were examined spectrographically during 1935, i.e. at a time too late for inclusion of the results in the articles on British Bronze Age and Harappan faience beads, although an appended note in the former drew attention to the fact that the composition of the Wiltshire specimens and certain of those from Tell el Amarna were identical, and therefore that the former had been derived from Egypt (12).

Unfortunately the late war, and the death of Mr H. C. Beck, resulted in the loss of the original spectrograms prepared by Dr Ritchie. Certain extracts from these in tabular

⁶ *Archaeologia*, LXXXV (1936), 207.

⁷ *Ibid*, 203.

⁸ *Ibid*, 225 and pl. LXIX, fig. 2, 1.

⁹ M. S. Vats, *Excavations at Harappa*, I (1940), 406 and 417; II, pl. CXXXIII, fig. 6 b and c.

¹⁰ *Ibid*, 434.

¹¹ *Ancient India*, no. 3 (1947), 123, pl. LJ, 21, 22.

¹² *Archaeologia*, LXXXV (1936), 252.

SECOND FIXED POINT IN CHRONOLOGY OF HARAPPA CULTURE

form were, however, fortunately preserved, and the relevant results are shown in the appended Table.

The apparatus used and an explanation of this method of analysis was contributed by Dr Ritchie to their paper on 'Far Eastern glass: some Western origins' by C. G. Seligman and H. C. Beck (13), and in which the method would appear to have been used for the first time for archaeological purposes (14). In Dr Ritchie's words 'The results are presented on a very roughly quantitative, as well as an accurately qualitative basis. In the table, L indicates a *large amount* of the element ('raies ultimes' very strong, and many other arc lines present), M indicates a *moderate amount* of the element ('raies ultimes' distinct, and a few other arc lines present), T indicates a *trace* of the element (spectrum practically confined to faint 'raies ultimes'), and O indicates that the element is absent, or present only in infinitesimal amount. While accurate figures cannot, of course, be assigned to these results, it may be taken that L indicates quantities of the order of 30 per cent, M of the order of 10 per cent, and T of the order of 1 per cent or less'.

The table shows the composition of the core of each bead and, where this was possible, that of the glaze also. It will be immediately apparent that the composition of all the beads is not the same. On the one hand actual identity of composition of the Wiltshire and Tell el Amarna specimens indicates an identical source of origin; whereas both the Scottish segmented and quoit specimens, though identical to one another, are dissimilar, which may imply a different source and/or a different date. Further, in general the surface glaze in each case is very similar to that of the corresponding core, which is not really surprising since the former often tends to penetrate the latter.

For the matter in hand we should note the absolute identity between the beads from Harappa and Knossos, whereas that from Ur is dissimilar in that it contains traces of silver, chromium, manganese and nickel. The bead from Tell el Amarna differs only in containing traces of lithium and tin, and less calcium; but whether these latter differences are significant or not further extensions of the work alone can decide.

Now this identity of composition of specimens from Harappa and Knossos can mean only one thing; that they were derived from the same source. Also that Sumer was not implicated other than possibly having acted as a trade or other route over which the beads were passed.

Though not exactly a typical bead of its type, but nevertheless segmented (15), the Minoan bead came from an unimpeachable horizon, from the Temple Repositories at Knossos which contained a rich variety of faience and other objects (13). In noting these beads from the Temple Repositories, Pendlebury observed that the segmented variety showed distinct Egyptian affinities, especially to those of the XVIIIth Dynasty (17). Whilst the deposit is of Middle Minoan III horizon, this apparent affinity with the XVIIIth Dynasty implies a late rather than an early date in that horizon, and we may not be far wrong in suggesting c. 1600 B.C. as the date of the Knossos and Harappa segmented

¹³ Published in *The Bulletin of the Museum of Far Eastern Antiquities*, no. 10, Stockholm, 1938.

¹⁴ See, however, the results of other spectrographic analyses by Dr Ritchie in a paper entitled, 'Spectrographic Studies on Ancient Glass: Chinese glass from pre-Han to T'ang times', *Technical Studies in the Field of Fine Arts*, v (1937), 209; and 'Egyptian Glass, mainly of the 18th Dynasty, with special reference to its cobalt content', M. Farnsworth and P. D. Ritchie, *ibid.*, vi (1938), 154.

¹⁵ *Archaeologia*, LXXXV (1936), pl. LXIX, fig. 2, 9.

¹⁶ J. D. S. Pendlebury, *Archaeology of Crete*, 1939, 165 ff.

¹⁷ *Ibid.*, 173.

ANTIQUITY

beads in question (18), and possibly of other faience beads in the same contexts, many of which are stated to have been found in the superficial layers of Cutting HP xxx (19).

Here then it is suggested we have a second fixed point in the chronology of the Harappa Culture which reinforces Wheeler's and Piggott's deductions. We must of course allow the lapse of some decades for their transference to the Indus, either by a land or sea route ; but we can, it is suggested, assign with confidence a date of c. 1550 B.C. for their appearance on these Indus sites, which certainly confirms the generally accepted approximate date for the composition of the Rigveda, and of contact by trade or otherwise with Europe (20).

In conclusion, however, I must emphasize that these spectrograms were derived from single specimens of beads only, though the inclusion of separate determinations, where possible, of core and glaze corroborate one another ; and that the original investigation was in the nature of an exploratory survey of the possibilities of this method of approach. Clearly, verification of the findings and extension of the work is necessary, but there can be little doubt that we have here a most valuable method applicable to archaeological research.

¹⁸ Ibid, 175.

¹⁹ *Ancient India*, no. 3 (1947), 123.

²⁰ See, for instance, H. Peake and H. J. Fleure, *Merchant Venturers in Bronze*, 1931, 124-137.

TABLE
SPECTROGRAPHIC ANALYSES OF BLUE FAIENCE SEGMENTED BEADS

	Na	K	Li	Ca	Ba	Sr	Pb	Bi	Sn	Ag	Cu/Zn	Mg	Fe	Cr	Mn	Al	Si	V	Ti	B	P	As	Sb	Ni	Co	Au	Hg	Cd
Tell el Amarna ..	Glaze	T	O	T	O	O	O	O	T	O	T	T	T	O	O	O	L	O	O	O	O	O	O	O	O	O	O	O
	Core	T	O	T	O	O	O	O	T	O	T	T	T	O	O	T	L	O	O	O	O	O	O	O	O	O	O	
Wiltshire ..	Glaze	T	O	T	O	O	O	O	O	O	T	T	T	O	O	T	L	O	O	O	O	O	O	O	O	O	O	
	Core	T	O	T	O	O	O	O	T	O	T	T	T	O	O	O	L	T?	O	O	O	O	O	O	O	O	O	
Scotland	Core	T/M	T	O	T/M	O	O	O	T/M	O	T	T	T	O	T	T	L	T	O	O	O	O	O	O	O	O	O	
Scotland 'Quoit'	Core	T/M	T	O	T/M	O	O	O	T/M	O	T	T	T	O	T	T	L	T	O	O	O	O	O	O	O	O	O	
Knossos	Core	T	O	O	T/M	O	O	O	O	O	T	T	T	O	O	T	L	T	O	O	O	O	O	O	O	O	O	
Harappa	Glaze	T	O	T	T/M	O	O	O	O	O	T	T	T	O	O	T/M	L	T	O	O	O	O	O	O	O	O	O	
	Core	T	O	O	T/M	O	O	O	O	O	T	T	T	O	O	T	L	T	O	O	O	O	O	O	O	O	O	
Ur	Glaze	T	O	O	T/M	O	O	O	T	T	T	T	T	O	O	T	L	O	O	O	O	O	O	O	O	O	O	
	Core	T	O	O	T/M	O	O	O	O	T	T	T	T	T	T	T	L	O	O	O	O	O	O	O	O	O	O	

L = of the order of 30 per cent or over.
M = of the order of 1-10 per cent.
T = of the order of 1 per cent or less.
O = not detected.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- THE FOUNDERS OF THE ZIMBABWE CIVILIZATION, by G. A. WAINWRIGHT. *Man*, June, 1949, No. 80 (Vol. XLIX, 62-6). [The author attempts to prove, from archaeological and documentary evidence (Mas'udi) that Zimbabwe was founded in the 9th or 10th century A.D. by Gallas migrating southwards from the Harar region of Ethiopia, where they had learnt to build in stone].
- A HUNDRED YEARS OF WELSH ARCHAEOLOGY: centenary volume of the Cambrian Archaeological Association, edited by V. E. NASH-WILLIAMS. John Bellows Ltd., Gloucester, 1949. 21s (postage 6d). [An admirable up-to-date outline of Welsh early history: prehistoric period, 56 pages, by W. F. GRIMES: Roman period, 25 pages, by H. J. RANDALL: early Christian and medieval, two parts each, 43 pages: good index, many illustrations].
- RIDGE AND FURROW AND THE OPEN FIELDS, by M. W. BERESFORD. *Economic History Review* 2 Ser. Vol. 1, No. 1, 1948, 34-45 [Shows by a correlation of air-photos and old cadastral plans that the ridged fields so common in the Midlands are simply the intact 'fossilized' remains of the medieval field-system, and emphasizes the need for historical field-work].
- THE WESTERN GREEKS: the history of S. Italy and Sicily from the foundation of the Greek colonies to 480 B.C., by T. L. DUNBABIN. Oxford University Press, 1948.
- BRITISH ARCHAEOLOGY: a book-list for teachers. Council for British Archaeology, c/o Inst. of Arch., Inner Circle, Regents Park, N.W.1. 1s 6d. [Indispensable authoritative guide to the best that has been written on all aspects, from the Old Stone Age to the Industrial Revolution].
- BRITONS, ROMANS AND SAXONS ROUND SALISBURY AND IN CRANBORNE CHASE, by C. F. C. HAWKES. *Arch. Journal* CIV, 1948, 27-81. [A masterly account of General Pitt-Rivers' excavations; by using the General's own evidence, interpreted in the light of 50 years' archaeological progress, Professor Hawkes is able to reach new and important conclusions. He pays homage to the genius of the General whose methods made it possible to achieve this].
- THE PREHISTORIC AND ROMAN REMAINS OF FLINTSHIRE, by ELLIS DAVIES: published by the author at Whitford Vicarage, Holywell, Flintshire. £1 1s 0d. [A companion volume to the author's *Denbighshire*, published 20 years ago, and as valuable. It gives a complete descriptive list of everything in the county, and will be of the greatest use to all future students. One wishes that similar compilations could be made of every county].
- FOSSATUM AFRICAE: vue aerienne de l'organisation romain dans le sud algerienne, par J. BARADEZ. Arts et Metiers graphiques, 18 rue Seguer, Paris 6^e. [An air-survey copiously illustrated of Roman roads, forts and fields].

Notes and News

A MESOLITHIC HABITATION-SITE IN YORKSHIRE

A site of outstanding importance has been excavated this summer at Star Carr, Seamer, a few miles S. of Scarborough, by Dr Grahame Clark, who has kindly contributed the following :—

Excavations, carried out this summer with the support of the Prehistoric Society and of the Department of Archaeology and Anthropology of Cambridge University in the Vale of Pickering, Yorkshire, have revealed what promises to be the richest Maglemosian station ever found. Credit for revealing the possibilities of the Flixton area belongs to Mr John W. Moore, who first discovered Maglemosian flints and then succeeded in tracing them to their parent deposits. The site chosen for excavation on a more extensive scale was chosen with the explicit aim of recovering traces of the fauna on which the Maglemosians lived and as wide a range as possible of their material culture, including objects made from substances normally lost to archaeology through decay. Aided by the exceptionally low level of the water, the excavators, comprising a party from Cambridge, supported by helpers from the Scarborough region, were able in just under four weeks to examine approximately 500 square feet of the mesolithic level which was sealed by several feet of peat of early post-glacial age. From this single cutting, which exhausts only a small fraction of the site, around ten times as many 'harpoons' of Maglemosian type as have hitherto been found in this country were recovered, together with numerous other objects made from antler, a few from bone, a flint industry in mint condition, small stone beads of unique type, red ochre, amber ornaments and numerous tightly rolled spools of birch bark, resembling those from the Swiss lake villages and from the recent folk culture of the Alpine and Scandinavian areas. It may be added that some of our 'harpoons' are over a foot in length.

For the first time a substantial fauna has been recovered. According to a preliminary report by Dr F. C. Fraser and Miss D. E. King of the British Museum (Nat. Hist.), red deer with antlers much more powerfully developed than those of deer still extant in these islands abounded, but elk, roe deer and large ox were also common; young beavers were present in numbers and badger, marten and fox were also represented. It is of great interest that many of the deer antlers had been utilised to provide material for implements and that the technique of removing splinters by means of flint burins should resemble closely that practised by the Hamburgian reindeer hunters of Meiendorf. One of the unexplained features of the find is the presence of a series of male deer frontlets having the stumps of the antlers carefully shaped and the parietal bones perforated in three places. Among the flint implements burins are indeed the commonest type, though microliths and scrapers are common, and small, poorly made core axes are present.

Of even greater interest were the indications of a stabilizing of the surface of the gravel ridge on which the people lived, consisting of a mat-work of birch stems, antlers and brushwood. Further investigation of this feature, which may throw light on the early origin of crannogs and kindred structures, is very greatly needed. It is also important to obtain a wider range of the Maglemosian equipment and to take advantage of the exceptional conditions of preservation offered by the site. It may be mentioned

ANTIQUITY

that birch leaves at the archaeological level were perfectly preserved when discovered and that the fungi adhering to the birch stems were still intact. Samples of the peat were collected by Dr H. Godwin, F.R.S. and by one of his research assistants on the site and it is hoped that analysis of these in the new Sub-department of Quaternary Research at Cambridge should enable the ecological context of the occupation to be securely fixed.

EXCAVATIONS ON BLEWBURTON HILL, BERKS*

Three seasons' work (1947-9) on this hill-fort have proved both its existence (which had been doubted†, though on quite inadequate grounds) and age. An account of the first season's work, written by Mr A. E. P. Collins, is published in the *Berkshire Archaeological Journal* (1); for notes on the results of 1948 and 1949 we are indebted to Mr W. A. Smallcombe, Curator of the Reading Museum, which, together with the Berkshire Archaeological Society, was responsible for the work throughout.

Blewbarton Hill is a whale-backed outlier composed of Lower Chalk, and is separated from the main escarpment of the Berkshire Downs to the south by a narrow strip of Upper Greensand. It lies between the villages of Blewbury on the west and Aston Upthorpe on the east, whose parish boundary passes right through the middle of the hill-fort. This boundary is of Anglo-Saxon age and the hill was called *Bleobyryg dun* in A.D. 964 (2), showing that the modern form is corrupt and should properly be Blewburdon. The first element is O.E. *bleo*, colour, used adjectivally. The same combination occurs in the name of Bleadon in Somerset, and is explained by Ekwall as descriptive of the variegated appearance of the hill, caused either by its vegetation or soil or by both. If, as is probable, the hill was already under plough when the name was given, the hill would be conspicuous by reason of its creamy white chalk soil, contrasting with the duller hue of the surrounding plain. Cholsey Hill, 2 miles to the N.E., is a similar chalk outlier whose upper part, as seen from the train, has just this creamy white appearance (3). The Icknield Way, certainly a prehistoric road, passes half a mile to the south.

A feature of geological interest is the capping of hard ochreous gravel containing quartzite pebbles derived ultimately from the Triassic Bunter sandstone, whose nearest outcrop is now near Birmingham. A scattering of these pebbles is found also on the spurs of the main chalk ridge 3 miles to the SSE, west of the Thames between Moulsoford and Streatley. They are regarded as of fluvio-glacial origin, and show that the great ice-sheet of the maximum glaciation extended at least its influence beyond the present Thames valley. Such 'erratic' pebbles are exceedingly rare elsewhere south of the Thames, if indeed they occur at all.

Blewbarton Hill is a familiar landmark to all those who rambled over this pleasant vale before it was invaded by the scientists of Harwell, the gravel-merchants of Dorchester

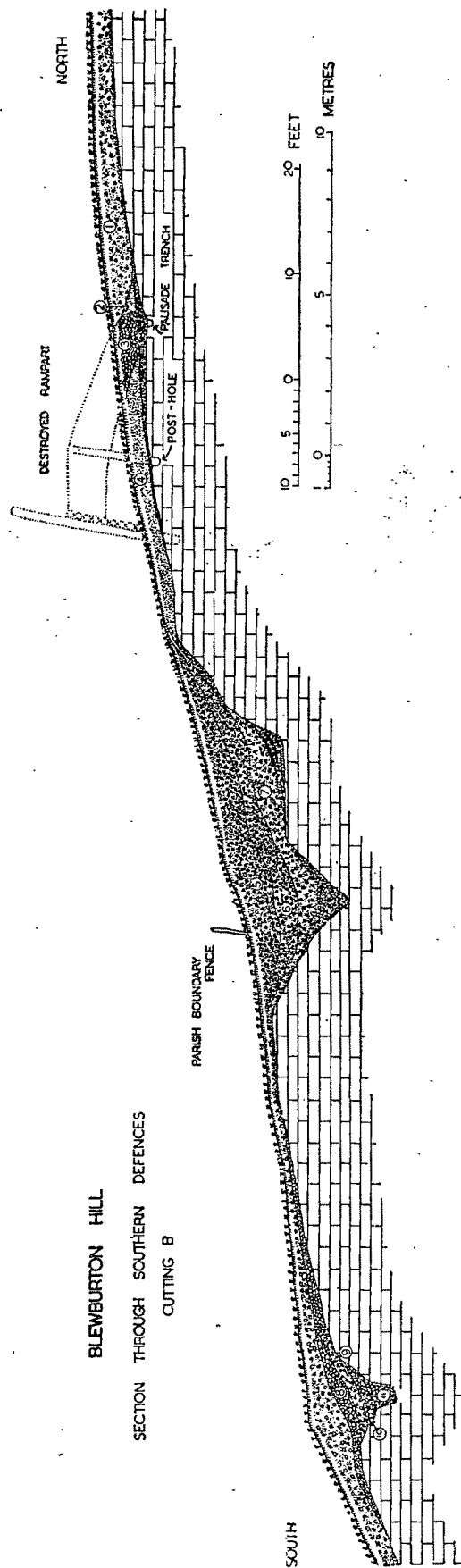
* The section (p. 209) is reproduced from the Report by kind permission of the Berkshire Arch. Society and Mr Collins.

† H. J. E. Peake: Presidential address to the Newbury District Field Club, 28 Jan. 1933: *Transactions*, 1930-3, 219-27.

¹ Published by the Berkshire Archaeological Society and printed by Messrs Lamport Gilbert and Co., 3 and 4 Gun St., Reading: vol. 50 (for 1947), 4-29.

² Birch, *Cartularium Saxonicum*, no. 1143.

³ It is worth recording that there may have been another hill-fort on a spur of the downs called on an old map Silsbury Hill, 1 mile S. of Cholsey, if the second element is O.E. *burh* (as it should be) and not O.E. *beorh*, barrow.



Section through southern defences, Cuttings A and B. Key to Stratification :

- | | |
|--|--|
| <ol style="list-style-type: none"> 1. Plough soil full of chalk fragments and flint and quartzite pebbles 2. Loose, dark 'occupation earth' containing AB sherds, etc. 3. Clean chalk rubble rampart capping 4. Loose black soil containing A sherds, etc. 5. Light brown plough soil full of chalk fragments | <ol style="list-style-type: none"> 6. Darker plough soil with chalk fragments 7. Brown plough soil with many chalk fragments 8. Compact angular chalk rubble 9. Yellowish soil with chalk fragments 10. Large chalk fragments in yellowish soil 11. Loose chalk rubble |
|--|--|

Turf-lines are shown in solid black

ANTIQUITY

and the aviators of Culham. Its chief feature is the series of step-like terraces or strip-lynchets formed on its slopes by centuries of cultivation during the Middle Ages. These have obliterated all outward signs of the rampart and ditch; but from the encircling plan they follow there could be little doubt that (as the Old English name showed) there was once a hill-fort there. Similar strip-cultivation penetrated and masked the defences of other hill-forts, such as Battlesbury (above Edington, Wilts) and Credenton Hill (near Banbury) (4).

The presence of a hill-fort on Blewburton Hill was proved during the first season. Not even centuries of ploughing had completely levelled the rampart, whose inner facing of hard chalk rubble, $3\frac{1}{2}$ feet thick, was found. Beyond this (outwards) was a layer, 2 feet thick, of loose black rampart soil where the burrowing of rabbits had destroyed all stratification. The rampart rested on a 'continuous black rather greasy layer' representing the turf-line of the old surface upon which it had been built. This black layer ran *unbroken* over a small trench $1\frac{1}{2}$ feet deep and 1 foot wide; which had once held a wooden Palisade. It is clear, therefore, that, before the rampart was constructed, the occupants of the hill had erected round it this wooden defence, and that between the making of this and of the rampart, enough time had elapsed for the Palisade to decay, the trench to fill up and the turf to grow over it again as before. The finds showed that the first, or Palisade, period was associated with pottery of Iron Age A type, and the second, or Rampart, period with pottery of Iron Age AB type. The central date is about 250 B.C. according to the Hawkes' chronology now generally accepted, so that we may date the first period about 400-250 B.C. and the second after 250 B.C., but very soon afterwards, continuing to perhaps about 100 B.C. (5). Beyond the rampart was the main ditch (38 feet wide and $10\frac{1}{2}$ feet deep) whose section was curiously abnormal, having a broad step on the inner scarp-face. This feature occurred also at Bury Hill, Hants, excavated by Professor Hawkes (6), and may on both sites have been merely an adaptation to natural conditions (a fissure in the chalk). Beyond the main ditch again, at a distance of 30 feet, was another ditch (9 feet wide and not less than 9 feet deep). The first of these two ditches obviously belonged to the rampart behind it, but the second could not be dated.

Within the area of the hill-fort were found several pits and post-holes dated by finds exclusively to the first (Palisade) period.

The finds included remains of both saddle and rotary querns, a bronze finger-ring of unique type and the usual collection of stone, bone and horn objects. The ring was the only metal object found.

Summing up the results of the first year's excavation, Mr Collins concluded that the first settlers belonged culturally to the Wessex A province, as indeed the haematite-coated bowls prove. Historically, it was this lovely burnished ware, often bright red in colour and pleasant to handle, that first attracted the attention of archaeologists, surprised and pleased to find such nice stuff instead of the uncouth food-vessels and collared urns of the barrows. It is now of course well known, but its recognition falls well within the archaeological memory of the present writer. It is the mark of our earliest Iron Age culture, and the Blewburton 'A' people who shared it seem (for reasons given in the report)

⁴ The existence of a hill-fort here is inferential only, but judging from the late Major Allen's air-photograph the inference is fully justified.

⁵ Except for the last these absolute dates are not specifically mentioned in the Report, but are inserted here by the writer for expository purposes only.

⁶ *Proc. Hants. Field Club*, XIV, 291-337.

NOTES AND NEWS

to have belonged to a rather late phase—if one put it between about 350 and 250 B.C. one would probably not be far out.

The succeeding AB culture is so called because it represents a modification of A by the bearers of the B. culture. This was introduced by Marnian invaders round about 250 B.C., and it was presumably the situation so created that made the Blewburton people make a rampart round the hill. In this they were doing what most of the other hill-folk had already done in the 'war scare' of the invasion, e.g. at Quarley and Little Woodbury, where palisades were superseded by ramparts, and at Ladle Hill where (probably, for it has not been excavated) a hill-fort was then begun but never finished. The preventive measures taken appear to have been successful, for the Marnian invaders veered off and their chief settlement-areas (of the B culture) are in the east and northeast. One group, the Parisii (from the Paris region) settled in Yorkshire where they introduced the chariot-burials characteristic of the Marne district.

The conclusions drawn from the first year's work seem to have been confirmed by the later excavations, except that the big (inner) ditch was proved to have two periods. The main entrance, on the SW side, was explored, and the road leading into it was found to have a rough cobbled surface of Bunter pebbles; at some later date this road had been narrowed by building on either side of it a wall of dry stone, formed of coral rag (limestone) whose nearest natural outcrop is at Marcham, 9 miles away to the NW, near Abingdon. Similar limestone blocks were also found beside the road (but on one side only) at Buckland Rings (7), just before reaching the main gate. At Flewburton the post-holes of the main gateway have been found, and are remarkable for having held *half*-trees, not the whole trunk; they were several feet in diameter and depth.

A trench dug from inside through the rampart and on through the strip-lynchets on the west seems to have shown that the latter were medieval. Saxon burials were found within the rampart; it would be most interesting if these graves could somehow be related stratigraphically to the strip-lynchets. 'In the section through the main ditch outside the rampart . . . we unearthed a man, pony, dog, iron adze and pot', all associated and of Iron Age A.

We have given a rather lengthy summary of these excavations not because the site is necessarily any more important than many others, but because they are in many ways typical of the sort of work that is going on quietly all over the country, and because the site is easily accessible and probably well known to many readers already. We would like to add another reason, that the report published—itself admirably succinct and lucid and well illustrated—shows that the work is being well and truly done.

EARTHWORK AT OLD YEVERING, NORTHUMBERLAND

One of the chief incidents in the introduction of Christianity to Northumbria was the stay of Paulinus at King Edwin's 'villa regia' of Adgefrin about A.D. 630 (1). The identification of Adgefrin with Yevering seems never to have been doubted, but no convincing suggestion has yet been made as to the actual site of Edwin's 'villa'. Certainty is impossible without excavation, but the claim of the structure described in this note (2) seems strong. The arguments in its favour are set out below.

¹ *Proc. Hants. Field Club*, XIII, 150, plate VI, 1.

² Bede, *Hist. Ecc.*, ch. XIV.

³ 15 SE 2 in the New List of Native Sites in Northumberland *Proc. Soc. Ant.*, Newcastle, 4 ser., vol. XI, no. 4, 1947, pp. 140-79. There called North Yevering.

ANTIQUITY

The remains lie on the steep northern slope of Yevering Bell, a hill crowned by the largest native 'fort' in Northumberland. Just above the site, the bare scree of the higher slopes gives way to grass and bracken, and the slope of the hillside decreases to one in three. The 500 foot contour line passes through the area, the lower boundary of which is at present roughly coincident with a modern wall which forms the upper limit of cultivated land. The adjacent parts of this wall contain particularly large stones, apparently derived from the structure, which seems to have been extensively robbed. Some of the walls seem also to have been obscured by soil creep down the steep hillside.

The central feature is an enclosure about 100 feet square surrounded by a wall about 8 feet thick. The interior has been levelled by cutting into the hillside, and shows faint traces of heavily robbed buildings. Surrounding this is an outer square enclosure, of about 160 feet side, formed by a slighter but still substantially built wall. This is now visible only for part of its circuit, but it appears probable that it originally formed a complete square. Small terraced areas occur in the space between this and the inner wall at the south-east and south-west corners, and perhaps at the north-east corner also.

Two further walls surround the site, but these are generally of slight construction, being only 2 or 3 feet wide. Wall 3, counting outwards from the centre, encloses a roughly rectangular area about 250 feet by 180 feet, the central feature being placed rather to the west of its centre. The space on the eastern side of the central feature, between walls 2 and 3 and below the 510 foot form line, has been partly levelled but the slope is still considerable.

The space enclosed by the outermost wall is even less accurately rectangular, about 400 feet by 250 feet, and once again extends further to the east of the central structure than to the west. There is another partly terraced area between this wall and wall 3, at the west end of the enclosure and below the 510 foot form line. The wall along the upper edge of the outer enclosure is more substantial, but does not extend for the whole length from east to west. At its eastern end its line is interrupted by some traces of walls which appear also to have extended above the 540 foot form line to the foot of the scree, but which are so slight, ruined, and obscured by stones fallen from the hillside, that the plan cannot be worked out.

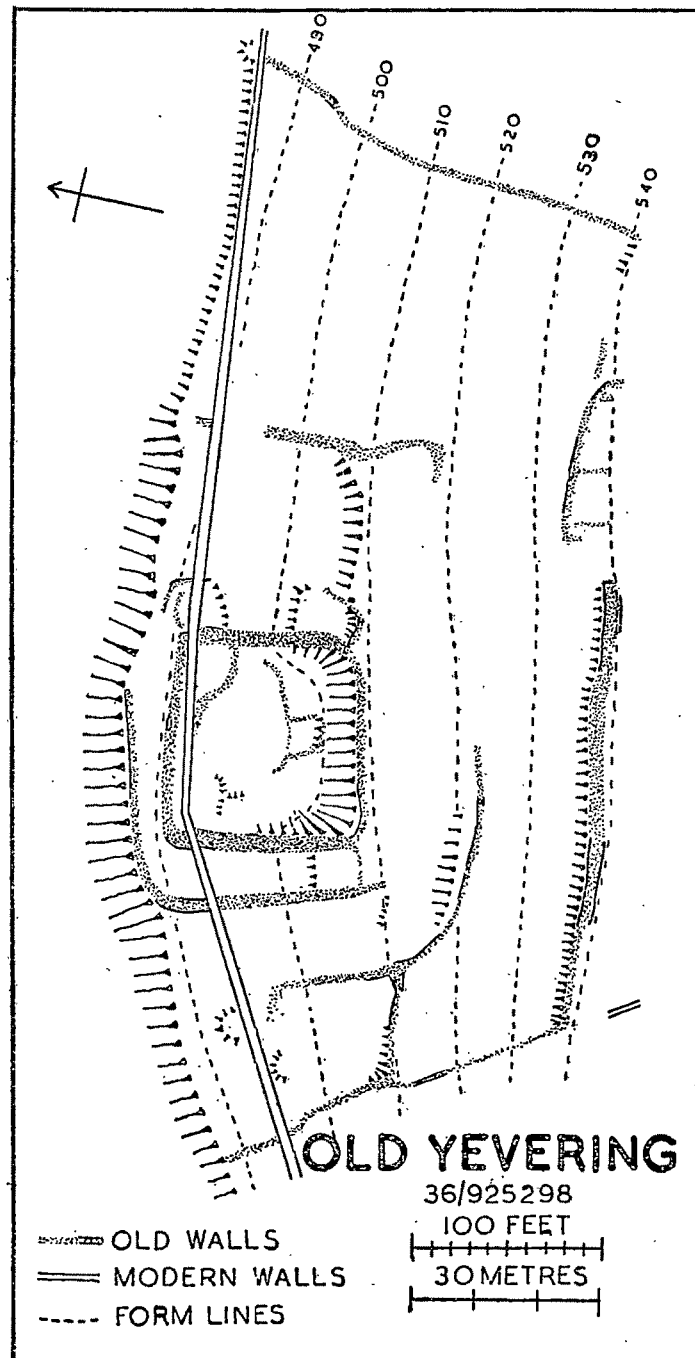
Walls 3 and 4 at present end on a negative lynchet which forms the upper limit of former cultivation, but in certain lights traces of what may be their continuation below this limit (3) are visible from the other side of the valley. It is possible, therefore, that the outer enclosures were also more nearly square in plan, and that the whole site was roughly symmetrical about its east-west axis.

The site has features which mark it out as distinct from the typical native homestead of the area. These latter are almost invariably terraced in this part of the Cheviots but the enclosed area usually lies at two levels, the upper carrying the huts, the lower presumably being a yard for cattle (4). Further, they are oval in plan, and concentric systems of walling do not seem to occur.

The site described is distinguished from these by the fact that each terraced area has been levelled to a single plane, the plan is essentially rectangular in outline, and the site is surrounded by four roughly concentric walls. It should be emphasized, however, that it is quite indefensible, and the walls can have served for no other purposes than as field boundaries or protection for cattle.

³ I am indebted to Sir Walter de L. Aitchison for this observation.

⁴ The recently published homesteads at Crock Cleugh, Roxburghshire, are typical. *P.S.A.*, Scot., LXXXI (1949), pp. 138-57.



ANTIQUITY

The claim that the site may be identified with the 'villa regia' of Adgefrin must now be considered. It is evident from Bede's account that this must be sought for on the northern slopes of the Cheviots, within reach of the River Glen, and somewhere within the neighbourhood of Yeving. It is possible that the modern village of Yeving covers the site, but the name Old Yeving does seem to imply some traditional recognition that an earlier centre of occupation was located somewhere near the site described. The 'traditional' identification of a medieval cottage about 430 yards north of the site with 'King Edwin's Palace' (5) should perhaps be discounted as an antiquarian fiction, but if accepted as genuine it provides strong support for the suggestion made here. It appears highly unlikely that the fort on Yeving Bell was occupied by Edwin. All the internal structures are circular, and excavations during the last century produced no relics later than the Roman period.

Turning to the site itself, its superficial appearance suggests that the labourers engaged on the work were familiar with the native technique of terracing into hillsides, whereas the designer responsible for the general plan seems to have been attempting something barbarically reminiscent of Roman layout. It may be noted that King Edwin used to have carried before him a standard, or *tufa*, apparently in imitation of Roman emperors (6). The character of the site is therefore not inconsistent with the identification proposed.

Final certainty can only be attained by excavation, but if this is indeed King Edwin's 'villa' it is an example, perhaps unique in this country, of a 7th century royal dwelling, on a site undisturbed either by later or by earlier occupation. As such, it is hard to overestimate its interest and importance.

A. H. A. HOGG.

SIXTH INTERNATIONAL CONGRESS FOR THE HISTORY OF SCIENCE

The Union Internationale d'Histoire des Sciences and the Académie Internationale d'Histoire des Sciences have agreed to ask the Dutch branch of the Union, the Genootschap voor Geschiedenis van Wiskunde, Geneeskunde en Natuurwetenschappen, to organize the Sixth International Congress for the History of Science in Holland.

This congress will take place August 14th to 20th, 1950, in Amsterdam University. Apart from general and committee meetings, the congress members will meet in four sections :

- (a) History of mathematics, physics and astronomy
- (b) History of chemistry, pharmacy and biology
- (c) History of applied science and technology
- (d) History of medicine.

The meetings of the last section will also form the Congress of the Union Internationale d'Histoire de la Médecine, which will plan the programme of this section. All papers to be read at this Congress should reach the secretary before May 1, 1950, and should be accompanied by a summary of 100-150 words. *After May 1st no further papers will be accepted.*

Further circulars will contain details on registration, payments, accommodation, entertainments, excursions, programme, etc. Provision will be made to entertain ladies or relatives accompanying members. The registration fee of fl.25.- (or the equivalent in other currency) for each member (fl.15.-, or the equivalent in other currency for each

⁵ See for example W. W. Tomlinson. *Comprehensive Guide to Northumberland*, p. 504 (11th ed.).

⁶ Bede, *Hist. Ecc.*, ch. xvi.

NOTES AND NEWS

of his guests) will cover the expenses of transactions of the Congress, transportation and light refreshments during the Congress as well as the final dinner. Hotel accommodation can be provided for fl.7.50.-fl.12.50 including breakfast; the daily general expenses need not exceed fl.25.- or the equivalent in other currencies.

Letters of registration, enquiries and suggestions should be addressed to the Secretary of the Congress: Prof. R. J. Forbes, Haringvlietstraat 1, Amsterdam-Zuid, The Netherlands.

'VASES SUPPORTS'

The *vases supports* of the French and Channel Island neolithic and megalithic cultures have been the subject of study and review by Mrs J. Hawkes (*Archaeology of the Channel Islands*, vol. II, The Bailiwick of Jersey, 1939, 72). Whilst distinguishing

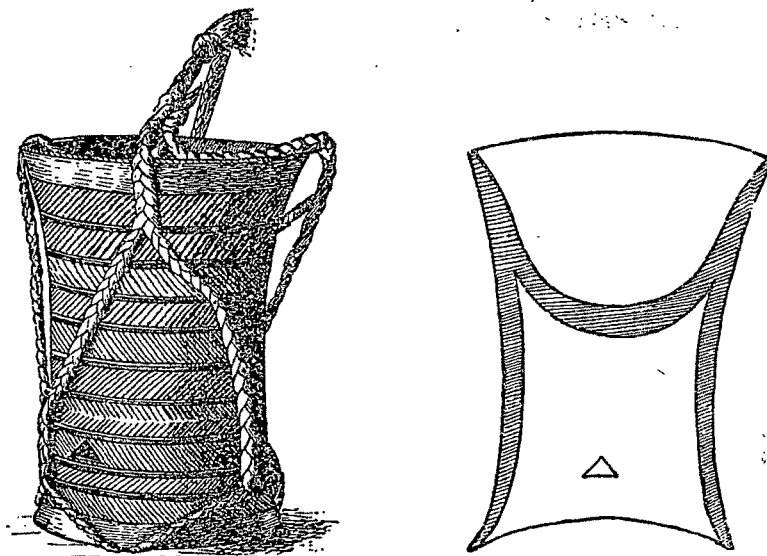


FIG. 1. VESSEL USED IN FUMIGATING SKINS, FROM VICTORIA NYANZA (†)

between the saucer and tubular types she points out that their function cannot have been the same throughout the whole area, the tubular type being predominant in the east at such camps as Chassey and Harrouard, where they were presumably used mainly for the domestic purpose of supporting round bottomed pots. In Brittany and Jersey on the other hand the saucer type is clearly associated with the megalithic complex and in that context has been interpreted as having been used for some such ritualistic use as a lamp or to receive ritual offerings. In support of this suggestion Mrs Hawkes has drawn attention to certain *vases supports* from Hougue Bie which were undoubtedly charred on the upper surface of the saucer.

The purpose of this note is to draw attention to another possible use for such objects. Whilst recognizing the danger of drawing too close an analogy from the equipment of comparatively modern African natives the resemblance of these saucer *vases supports* to the clay vessels used for fumigating skins by certain Kisiba tribes of the Victoria Nyanza appears to be close enough to warrant notice. Paul Kollman in his description of the region (*The Victoria Nyanza*, 1899, 91) states that 'there is an interesting vessel

ANTIQUITY

of black baked clay ornamented with neat line patterns. It is 8 inches high, and consists of two divisions as may be seen in the diagram of a section [Fig. 1]. In the upper part, which is cup-shaped, are placed bits of wood soaked in grease, with lighted shavings. The whole is then placed underneath a hide stretched out for drying, so that the smoke generated may cause the hide to lose its smell more quickly. The natives call the contrivance *kishwa*.

Comparison of the illustration of this African vessel (Fig. 1) with those illustrated by Mrs Hawkes from Jersey is striking, especially those from the Dolmen des Géonnais (Fig. 2a) and from Le Pouquelaye de Faldonet (*op. cit.* 227 and 235). But in certain respects some of the vessels illustrated by Le Rouzic from Er-Lannic bear an even closer resemblance (*Les Cromlechs de Er-Lannic*, 1930, nos. 1, 44, 50, etc.). Leaving aside the matter of ornament which could hardly be expected to be the same at periods so remote chronologically, one is immediately struck by the identical shape and position of the

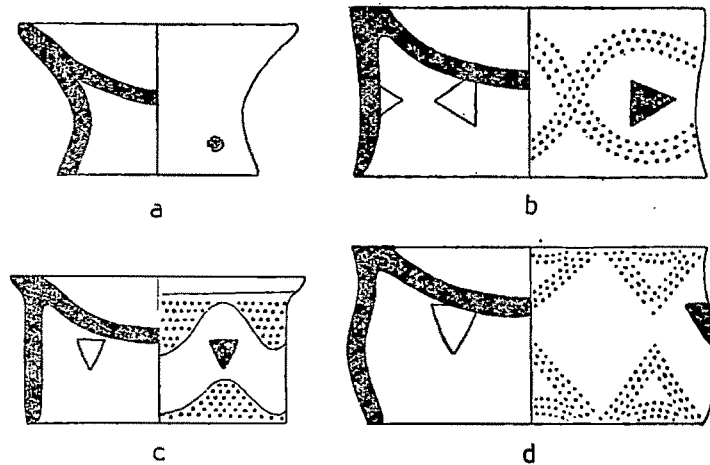


FIG. 2. 'VASES SUPPORTS'

(a) from Dolmen des Géonnais after Mrs Hawkes, (b, c and d) from Er-Lannic after Le Rouzic (4)

triangular perforations in the lower half of some of them (Fig. 2b, c and d). The object of these holes, or for that matter the simpler perforations so frequently encountered in the saucer type of *vases supports*, is not immediately apparent. As they cannot have contributed in any way to form a draught in the upper compartment we must presume that they were added to act as an escape for the heated air beneath, which otherwise would in certain circumstances have built up sufficient pressure to cause the support to move or even topple over. In any event it is now difficult to surmise why such comparatively complicated triangular excisions should have been adopted in preference to simple perforations. And to find a virtually identical technique practised some 4000 years later makes the matter even more surprising.

Such curious vessels may have had an independent origin, but the number of characters they possess in common suggests that they were invented for the same or a closely allied purpose. At present, however, we cannot be too sure of an independent origin in view of the very probable North African derivation of the West European neolithic culture, and the strength of tradition.

J. F. S. STONE.

NOTES AND NEWS

EXCAVATIONS AT WHITHORN

MR RALEGH RADFORD writes :—At the suggestion of the Society of Antiquaries of Scotland and the Dumfriesshire and Galloway Natural History and Antiquarian Society the Ministry of Works has begun excavations at Whithorn. The roofless nave and the eastern part of the medieval church, which stand in a large modern churchyard, have long been under the care of the Ministry, which is also Guardian of the Chapel on the Isle of Whithorn and the Cave at Glasserton, the two other sites traditionally associated with Saint Ninian.

Excavation was first directed to the nave of the Priory Church and revealed interesting data concerning the history and development of this building, the earliest part of which goes back to the middle of the 12th century. The interior, which had been disturbed in medieval and modern times, yielded no trace of any remains earlier than the 12th century. Attention was next given to the east end of the Priory Church, where ancient structures were uncovered by the Marquess of Bute at the end of the 19th century. Two buildings were represented by dwarf walls carried up 1 foot above the surface in frankly modern masonry. As no record of the character of the walls was available, it was thought desirable to reopen the excavations made by Lord Bute. The northern of the two groups of walls proved to be of late medieval date and probably formed part of the Infirmary of the Priory. The other building lay at the east end of the church and the walls had been cut through by the foundations of the crypt, a building erected about 1200 and forming part of an extension of the Romanesque church carried out in the early 13th century. The three older walls formed part of a small rectangular building which measured internally 16 feet wide and over 14 feet long. The levels shewed that the western end had been entirely destroyed when the crypt was erected. The walls were 4 feet wide and stood to a maximum height of 3 ft. The masonry was of local stone, roughly split into large irregular blocks and set in clay. The external face was usually formed by a natural line of cleavage along one of the bedding planes of the rock. Small stones were used to trig the larger blocks into position. The lowest courses of the south and east walls were undisturbed. The next courses appeared to be original but some of the stones had been moved slightly ; this may have been due either to removal and replacement at the time of the 19th century excavations or to earlier disturbance. Above these courses was modern masonry with roughly dressed stones set in mortar and clearly dating from the 19th century. The north wall had been entirely demolished at that time. The site was then used for a pit in which the bones found in the course of the excavation were buried and the wall roughly rebuilt out of the original material. It soon became clear that the walls had been explored by trenches driven along the face. These trenches contained stones, rubbish and a certain amount of mortar, very similar to that used in the 19th century walling. On the east face it was found that the trench stopped 6 inches above the base of the old wall and on removing the undisturbed soil the same mortar was found *in situ*, daubed on to the outer face of early masonry. No similar evidence was available for the inner face of the wall and old mortar was not found within the building. The evidence therefore points to a dry built structure the outer face of which was plastered with a pale cream coloured mortar. The date of the building is demonstrably earlier than 1200 but the archaeological evidence does not give any further clue to its date.

Excavations were also carried out within the Chapel of St. Ninian on the Isle of Whithorn. A small fragment of masonry earlier than the 13th century walls was found near the east end of the building, but the whole site had been so badly disturbed in modern times that no conclusions were possible. Trial excavations carried out in 1948 had

ANTIQUITY

demonstrated that the appearance of rectangular structures noted on the summit of the island, north of the chapel, was in part caused by natural ridges of rock and in part by tipping of debris; this probably came from the chapel as a fragment of late medieval pottery was found under the spread of rubble. It should further be recalled that no crosses, comparable with the early series from Whithorn itself and from St. Ninian's Cave, have been found on the Isle.

Technical considerations shew that the early building discovered at Whithorn is not of the period of Anglian supremacy (7th-9th century) and suggest that it is unlikely to be of the later period when there was a flourishing school of stone carving on the site. The remains inevitably recall Bede's description of *Candida Casa* with its church of stone, where St. Ninian, a Bishop of the Britons, lay (*Hist. Eccl.* III, 4). The date of this Saint, who is credited with the conversion of the southern Picts, is obscure. Opinion has tended to concentrate on the story of his visit to St. Martin (ob. 397) at Tours. But this is first recorded in the 12th century life by St. Ailred; the 8th century *Miracula Nynie*, a Whithorn production, knows nothing of the story and as Dr Levison has pointed out it is unthinkable that the poet would have omitted a personal link with the celebrated Bishop of Tours, if he had found it in his source (ANT., XIV, 287). There is however another indication in the story common both to the Life by St. Ailred and the *Miracula*, and so probably taken from the lost earlier life which both use. This is the episode of the blinding and subsequent healing by the saint of King Tudwal. Chadwick (*Anc. Scotland*, 146) has pointed out that the fourth genealogy in Harleian 3859 probably belonged to Galloway. Merfyn Mawr, the only datable member of this family, was killed in 681, according to the Annals of Ulster. Reckoning 30 years to a generation this would give a date about 450 for the first Tudwal in this family (Jesus College 20 suggests that one generation may have fallen out of the Harleian pedigree, and this has been allowed for). This date coincides with the period to which the earliest inscription, at Whithorn, the Latinus stone, should be ascribed. It is also conformable to the sources used by Bede, which dated St. Ninian considerably earlier than St. Columba, while the presence of Christians in the western Lowlands at this date is confirmed by the Epistle of St. Patrick to King Ceretic.

LONG BARROW NEAR PITSFORD, NORTHANTS

This tumulus* was first recognized as a long barrow by William Stukeley in 1724. In his *Itinerarium Curiosum* he mentions a 'long barrow at Pesford called Longman's Hill (1)'. Previously, in 1712, the Rev. John Morton had written, 'The Hill called Longman's Hill nearer the town of Pisford by the Northampton Road, being of an oblong shape about ten yards wide and not encompassed with a ditch I incline to think was a tumulus or Sepulchral Hill, till I hear of a likelier account of it . . . (2)'.

Among the papers of Sir Henry Dryden in the Northampton Public Library is an account of a 'Roman Tumulus' situated in Brampton Lane, Pitsford, which was investigated in August 1882. It is stated that 'Roman' urns and a spearhead were found in an area ninety by twenty feet but no further information about the area is given, nor of the depth at which the objects were found. In view of the measurements, however, it is possible that the tumulus is the one described by Morton and Stukeley.

* National Grid 752/678, one-inch O.S. 1946, no. 133.

¹ *Itin. Cur.*, 1724, p. 35.

² Morton's *Nat. Hist. Northants.*, 1712, p. 548.

NOTES AND NEWS

The tumulus has been marked on Ordnance Survey maps of Northamptonshire since the first edition of the one-inch map in 1815. On a map published in 1854 the site is marked as Lyman's Hill (3).

The site was visited on June 26th, 1948. The mound is situated four miles north of Northampton, about a hundred yards along the lane leading to the village of Pitsford from the main road from Northampton to Market Harborough. It is on the north side of the lane, and is at present fenced off. A number of modern houses flank the ends of the barrow, and there are also a few on the north side. A hedge at the side of the roadway cuts into the barrow on the south side.

The countryside is undulating and comparatively open, and the mound itself is about 350 feet above sea-level. The geological formation is that of the Inferior Oolite. At the time when the barrow was built the climate was probably considerably wetter than at present, and the area may have been more wooded. The water-supply would have been plentiful, as two streams, one flowing to the north of the site and another to the south, converge at a point about a mile and a half to the west of the spur on which the barrow is built. There are numerous springs in the district, and according to Morton, about 200 years ago there were 300 springs in the 1200 acres of Pitsford field.

Detailed measurements could not be obtained as the mound is planted with trees (4). The mound is oblong, measuring about a hundred feet in length and is about thirty feet broad. It is seven to eight feet high at the east end, and five to six feet at the west. The longer axis lies approximately east to west and is roughly parallel to the road. The east end has been disturbed and is spread about. The mound was originally longer, but a farmer is said to have removed a quantity of earth from the east end, and amongst this earth bones were found. It is not known whether the bones were human or not, but a human thigh-bone was picked up on the south side of the barrow: it has since been lost. If the barrow is the one investigated by Sir Henry Dryden, there is nothing to indicate that it was an extensive examination, as the main body of the mound is intact. The south side has, however, been broken into by the hedge. No megalithic features are visible and there are no signs of ditches. At present the mound is fenced off and is in no danger of interference.

It is still known as Lyman's Hill, and there is a local belief that it contains the remains of soldiers killed in the battle of Naseby.

There is every reason to regard this mound as a genuine long barrow, probably of the unchambered type. Although it has been marked as a tumulus on all Ordnance Survey maps since the first edition of the one-inch map of Northamptonshire (1815) it has not been described as a long barrow since William Stukeley, nor is it marked on any map showing the distribution of long barrows.

It cannot be said for certain that it is the same tumulus as that investigated by Sir Henry Dryden. There is no record of any scientific examination, but any bones, urns, etc., which have been found probably belonged to secondary interments.

If this barrow is, in fact, a genuine long barrow it is the first to be noted in Northamptonshire. The nearest unchambered long barrow is the solitary example on Therfield Heath, Royston, excavated by C. W. Phillips (5). A long barrow once existed in

³ *Archaeologia*, vol. 35, p. 394.

⁴ There are various references both to the time when the barrow was planted (e.g. Baker's *Hist. of the County of Northants.*, vol. 1, p. 65) and also to the hedge which has broken into the south side. (*ibid.*, p. 65).

⁵ *P.P.S.* 1935, pp. 101-7.

ANTIQUITY

Dunstable, and another has been claimed on Dunstable Downs (6). Shipley Hill in Leicestershire is another very doubtful example (7). In fact there does not seem to have been any extensive settlement of the midland area by long-barrow building people. The nearest large group of unchambered long barrows is that in Lincolnshire comprising fifteen examples, although there is a very large group of chambered long barrows in the Cotswolds. It may be, however, that an extensive field-survey of Northamptonshire would reveal other long barrows.

JOY FEREDAY.

PINS WITH ANIMAL OR SPIRAL HEADS

In a recent article in *Ancient India* (No. 4, p. 26 and following) Professor Stuart Piggott has discussed the importance of certain pins and a mace-head from Harappa and has traced their distribution from Tepe Sialk near Kashan to Harappa in the South and Mycenae in the West.

I am not concerned here with the mace-head but only with the pins—Professor Piggott points out that the presence of polychrome pottery of the Jamdat Nasr type and of inscribed proto-Elamite tablets would appear to date Sialk IV well back into the 4th millennium B.C. and goes on to say that the pins at Sialk must therefore antedate Cycladic or Trojan examples by at least a thousand years. He is only referring here to the pins with a double-spiral head but it is clear from his later remarks that he must assume a gap of some five hundred years between the Sumerian pins with animal heads and their earliest Western counterparts. Professor Piggott, however, has failed to notice the two bronze pins with bird heads from the first city of Thermi in Mytilene, a city contemporary with Troy I_a and b and therefore to be dated not later than the first quarter of the 3rd millennium B.C. in all probability. Thermi also had the pins with simple spiral heads one from the third city, and one from a mixed deposit. The Thermi pins bridge the chronological gap between the Sumerian and Cycladic pins but stylistically their strong geometric stylization looks more Caucasian than Sumerian.

Other animal-headed pins not noted by Professor Piggott are a bronze one with a head in the form of a sitting dog from Palaikastro dated at earliest perhaps early Minoan III*, and a bone pin found near Eretria and noted by me years ago in the Chalkis museum (museum inventory number 587 and referred to by Dr Lamb, Thermi, p. 166).

These bone pins from the Cyclades and Eretria are of interest in that they perhaps indicate that the whole group including Professor Piggott's animal headed rods were probably used as hair-pins for which no point is necessary, whereas pins required to pierce anything except a woollen garment would presumably require something better than a bone point, or an unpointed rod.

R. W. HUTCHINSON.

⁶ These are Mill Hill, Dunstable (now destroyed; *V.C.H. Beds.*, vol. 1, p. 150; *Arch. J.* 89, p. 174) and Pascombe Pit, which Phillips regards as a pillow-mound (*Arch. J.* 89, p. 150).

⁷ Stukeley's *Itin. Cur.*, p. 102, and Phillips in *Text to Map of Trent Basin*, published by the Ordnance Survey, 1933 (*Megalithic Survey of England and Wales*), p. 7.

* The pin was not stratified and can therefore only be dated stylistically.

Reviews

LIGHT FROM THE ANCIENT PAST: the archaeological background of the Hebrew Christian religion. By JACK FINEGAN. *Printed in the U.S.A. by Princeton University at Princeton, New Jersey.* [London: Geoffrey Cumberlege]. 25s. 1946

IN QUEST OF CIVILIZATION: a search for ancient clues to the modern puzzle. By RONALD LATHAM. *Jarrollds, London.* 21s. *N.d., but preface dated 1946.*

Both these books survey the ancient world, but they do so from very different angles. Mr Finegan's is frankly partisan; his avowed purpose is 'to give a connected account of the archaeological background of the Hebrew-Christian religion'. By judicious selection and omission the birth of Christ is made to appear as that 'one far-off divine event' towards which the combined efforts of mankind had been moving, since the days of the earliest food-producers. Because neither India nor China nor Greece nor the rest of Europe can be shown, even by the most wishful thinker, to have led up to 'the Hebrew-Christian religion', they are therefore disqualified. India has a single short paragraph; China and prehistoric Europe are ignored; and Athens is described, in the chapter 'Following Paul the Traveller', chiefly because it is the place 'where the apostle Paul once stood though but briefly' (p. 272). It is as though a future historian of the Nazi movement should describe Paris as the place where Hitler once stood (though but briefly). Indeed the book is open to many of the same kind of criticisms as those passed on German historical writings published during the Nazi régime, with the qualification that Mr Finegan's purpose is achieved by omission and selection and not by distortion or deliberate mis-statements of fact. Within the limits of its subject the book is both scholarly and up-to-date.

The beginning of 'modern archaeology' is assigned to the year 1798, 'when nearly 100 French scholars and artists accompanied Napoleon on his invasion of Egypt'. That was of course a landmark, but it represented no new development in technique. Inscriptions had been copied and monuments drawn and planned for many centuries before then. The present reviewer once asked the late Professor Sayce when, in his opinion, modern archaeology began, and he replied without a moment's hesitation 'With Schliemann'. That seems for many reasons to be a better view.

But these are trifles. What we object to is that the author should take a large chunk of human history and label it the background of a particular religion (nowhere clearly defined) which at the most prevailed over a very small (and otherwise quite unimportant) part of the whole area surveyed and for a few centuries only at that. It is as if a historian of 'the English-Christian religion' were to summarize the whole of European prehistory and history from the neolithic period down to the Reformation and call it the background of that religion. Has Christianity any more connexion with the builders of the Pyramids than English Protestantism has with the builders of Stonehenge? How can any impartial historian regard the one as, in any sense except perhaps a geographical one, forming part of the background of the other? We are reminded of those meretricious advertisements which display photographs of some famous place or building with expensive vehicles in the foreground.

Mr Latham's is a very different book—balanced, impartial and well written—so impartial indeed that it is often hard to discover what his own views are. Yet for all this

ANTIQUITY

impartiality the style is not arid. The 'book grew out of a course of lectures given . . . at the Working Men's College, St. Pancras, in 1938, and greatly enjoyed by the lecturer'. The author has the happy knack of telling one just what one wants to know about such things as the Egyptian system of writing—a knack unfortunately not possessed by many specialists. The period covered, after a preliminary introduction, is the two thousand years before the birth of Christ. The treatment is selective, divided into a first survey of the world (20th century B.C.) a second survey (6th century B.C.) and the classical age. Within these sections chapters are devoted to the chief centres of civilization. Adequate space is allotted to India, China and Greece. The history of Israel as recorded in the Bible is summarized with a rare and welcome objectivity. A final 'retrospect' of nine pages reveals the author as an original thinker as well as an expositor. There are no references to other than original authorities, and no 'list of books consulted'. But the discerning reader will perceive here and there the influence of some of the best modern authorities, whose writings have been so intelligently assimilated that particular acknowledgments have become superfluous.

The format is adequate but unattractive. Time and space are represented by a rather primitive map at the beginning and a useful chronological table at the end. (A minor point of criticism—on the map the 'Haddendoa' are given undue prominence and are slightly misplaced; they are merely one of many Beja nomad tribes). The illustrations reveal a quiet sense of humour. The frontispiece ('Food-production—the idyllic aspect') shows peasants cultivating a field ankle-deep in water; and an oblique air-photograph of 'the Promised Land' shows the Jordan meandering amid the scrub in its valley between the arid hills of the 'bad lands'. This is hardly fair! Every critic thinks he could improve upon an author's selection of illustrations; but those chosen have two merits at any rate—they are relevant to the text and most of them are unfamiliar. But the vast untapped source of official air-photographs, ideal for a book like this, is hardly used. (The mound of Erbil, of which an air-photograph was published in *ANTIQUITY*, vol. x, 1936, opposite p. 136, would have been a useful inclusion).

We can heartily recommend all readers of *ANTIQUITY* to buy this book. O.G.S.C.

A DESCRIPTION OF ORDNANCE SURVEY LARGE SCALE MAPS. *Published by the Director General at the Ordnance Survey Office, Chessington, Surrey, 1947. Price, 1s 6d.*

THE SAME OF SMALL SCALE MAPS. *Same price.*

The Ordnance Survey of Great Britain differs from the national survey-departments of some other countries in that it does not stagnate. Its Directors are 'always seeking something new, that which they have done but earnest of the things that they shall do'. The reason is to be found, we think, in a fine tradition of public service which goes back unbroken to Mudge, Colby and Roy; and in an awareness of what that service demands. There is here none of that complacency associated abroad with the scale of 1:80,000, for instance.

The outstanding new development today is the National Grid which (coupled with retriangulation, a magnificent achievement in itself) makes possible the production of a uniform series of maps from the 1:2500 to the 1:1,250,000. Archaeologists will chiefly welcome the new intermediate scale of 1:25,000 ($2\frac{1}{2}$ inches to the mile) which is ideal for much historical work and adequate even for field-work, though for the latter the 6-inch scale still remains the best. The small scale pamphlet reproduces a portion of the Ordnance Map of Roman Britain; and we understand that both this and the Dark Age

REVIEWS

maps will before long be reprinted and again available, together with some others of the same kind.

It is not the fault of the Ordnance Survey that its maps are nowadays more beautiful than the country they represent. O.G.S.C.

THE ROYAL ART OF ASTROLOGY. By ROBERT EISLER. *Herbert Joseph, London, 1946. 296 pages, illus. 18s.*

This book is an exposure of the ridiculous racket called astrology which has replaced Christianity as the religion or superstition of many uneducated English people, particularly women. The author shows how its 'professors' do not even know the history of their own art, and that their prophecies have been falsified over and over again. On pp. 21-3 is an amusing series of extracts from the lucubrations of some of the newspaper astrologers during 1939 and 1940. (On 21 April 1940, Lyndoe thought that an Allied victory would not be long delayed, and on 19 May Naylor forecast 'better news towards the weekend', during which Belgium surrendered). The factual parts of the book are generally sound, but the author gets out of his depth when he theorizes, as for example in his absurd explanation of the origin of writing and astronomy, based upon the statement of a writer who lived more than two millennia afterwards. This theory is solemnly quoted on the dust-cover as demonstrated fact. He does show, however, that there is no truth in such claims as that of Pearce (1911) that the learned men of the 19th century denied the truth of astrology without taking the trouble to investigate it, being too prejudiced to do so. After quoting by name those who have done so he concludes (p. 28): 'Not one item of the list of their books . . . is ever quoted by any one of the defenders of astrology. They will not acknowledge honestly the decisive fact that their futile practices have been investigated with the greatest care and impartiality by the foremost scholars of the leading western nations for now almost three centuries, and that not one of these has failed to condemn them as the stale, superstitious residue of what was once a great pantheistic religion and a glorious philosophical attempt to understand and rationally to explain the universe, a bold enterprise to which we owe not only the whole of our astronomical knowledge, but the most essential part of all our physical science'.

There are some amusing pictures, including one (on Plate 1) of Mrs Suchbury Hurren and Mr Alexander Ruperti explaining the horoscope of Jesus Christ. O.G.S.C.

NEW ZEALAND ARCHAEOLOGY AND AIR-PHOTOGRAPHY. By G. BLAKE PALMER. A paper read before the 6th Science Congress of the Royal Society of New Zealand, May 1947.

New Zealand would not appear to be a very promising country for archaeological air-photography, for much of it is covered with forest and scrub. It is therefore all the more creditable that an English doctor who has gone there should have been able to discover and publish such excellent results as those illustrated here. The *pa* at Waikaukau (Fig. 6), a rectangular earthwork with remains of habitations visible within it, is well revealed by a good air-photograph taken by the N.Z. Aerial Mapping Company, and so are the associated groups of pits and outlying earthworks. Other air-photos of the lagoons on the shore at Cloudy Bay at the mouth of the Wairau river in Marlborough confirm and amplify the extent of Maori canals 'constructed to facilitate the fixing of eel-weirs and the driving of moulting wild-duck'. Dr Palmer makes certain useful suggestions about the taking by the R.N.Z.A.F. of archaeological air-photographs—a

ANTIQUITY

practice long established in this country. He also advocates the marking of more of the fast vanishing Maori sites on the official maps, and the appointment of an Archaeology Officer, as at the British Ordnance Survey. Needless to say we heartily commend these suggestions to the powers that be there, with the hint that now is the time. *Bis dat qui cito dat.* O.G.S.C.

THE GREAT CHARTULARY OF GLASTONBURY. Edited by DOM AELRED WATKIN. Vol. 1: pp. LXXXIX, xviii, 236. Somerset Record Society, Vol. LIX, 1947. Printed for subscribers only.*

The Great Chartulary of Glastonbury is a 14th-century manuscript, written in a single hand and completed *circa* 1340. It has been edited by Dom Aelred Watkin and will be published in two volumes by the Somerset Record Society. The first volume has already appeared, and the second will be eagerly awaited by all who are interested in Glastonbury Abbey.

In Vol. 1 Dom Aelred Watkin has made easily accessible a carefully edited text of about half of this important chartulary. He warns us of certain defects. It is a pity that the text was not collated with the original documents when these exist. It is a pity that it was not possible to collate the text with the *Secretum Domini* (a copy of the Great Chartulary made for the Abbot of Glastonbury in 1342 and now preserved in the Bodleian Library), but we are assured that no readings of value have been lost by this omission. The editor cannot be blamed for the financial conditions that precluded the publication of the text in its entirety, and it should be noted that he omits or summarizes only what is printed elsewhere or readily lends itself to calendaring (e.g. formal confirmations, final concords, etc.). He always gives references and the reader is never left in doubt. Many would prefer fuller notes, but we cannot complain for the editor's object was to produce 'a careful and accurate text' and to avoid loading it with critical apparatus. Within his carefully defined limits he has done remarkably well. But it would have been better if, instead of inserting his emendations into the text and relegating the manuscript readings to footnotes, he had left the text as it stood and given the emendations as footnotes.

Within the chartulary itself documents are arranged in groups according to their content. The major groups now printed in Vol. 1 deal with Glastonbury's archidiaconal rights over seven neighbouring churches, appropriated churches, pensionary churches, the relationship between Glastonbury and the Bishop of Bath and Wells (important for the history of the abbey), the patronage of the Priory of Burtle (seven documents which bring together the little that is known of a small and obscure religious house), papal privileges, royal privileges, feudal services owed to the king, royal grants of markets and fairs, royal protections, the liberties of the forest, royal confirmations, knights' fees, Final Concords, and royal inquisitions concerning the origins of the abbey's numerous immunities and rights. Together they present a many-sided picture, legal, economic, historical and topographical, of the development of a great monastic establishment. They illustrate its relations with pope, king, diocesan, and dependents, and they also preserve much information of general importance. They form, in the words of the editor, 'a source-book preliminary to the history of Glastonbury'.

* The reviewer understands that non-members of the Somerset Record Society may become, upon payment of two guineas, 'temporary' members and so receive both volumes of *The Great Chartulary*. Those who wish to take advantage of this concession should communicate with the Honorary Secretary: Rev. Prebendary T. F. Palmer, East Brent, Somerset.

REVIEWS

Instead of an introduction the editor has supplied (pp. xv-LXXXIX) a fairly detailed 'Descriptive Analysis'. This is an excellent feature of the volume. It enables the editor to indicate the correct position of certain misplaced documents, to place others in a logical sequence within their groups, to discuss the significance of the more important documents, and to relate all of them to each other and to their background. This piece of apparatus, skilfully designed and effectively controlled, goes far towards making full textual notes unnecessary. From it the reader who does not wish to plod through the Latin text will quickly obtain a picture of the contents of the chartulary and of the great abbey which owned it. And the scholar also will find it extremely useful.

The second volume will contain that part of the chartulary which deals with Glastonbury estates in detail. From it we may expect much further information, especially topographical information. It is the part of the chartulary that will appeal most to archaeologists, local historians, and students of place-names. There will also be an introduction and an index of all names 'down to the smallest field names'. Dom Aelred Watkin has rendered a great service to scholars. He has also led us to hope that he will some day produce that history of Glastonbury for which he is now preparing the ground.

F. T. WAINWRIGHT.

THE PYRAMIDS OF EGYPT. By I. E. S. EDWARDS. 1947. Pelican Books, pp. 256, 15 plates and 34 line drawings. Price 1s.

A reliable book on Egyptian pyramids by a recognized authority has long been needed, and the appearance of such a book in the Pelican series is particularly welcome in these days of rising costs.

A short introductory chapter provides the necessary historical and religious background to enable the reader to consider the pyramids as an expression of the religious ideas which led to their construction.

The main body of the book (chapters I-VI) is concerned with a description of the development of the pyramid from the archaic mastabas to the degenerate pyramids of the Late Egyptian period. The disadvantage of such a method of treatment, in which 170 pages are used to follow the course of development of the royal tomb, is that the various and inevitable digressions lead to the risk of making it difficult to see the wood for the trees. None the less, the author has skilfully woven into his narrative the various important points, and has made several suggestive observations.

In describing the Step Pyramid complex, he points out (p. 53) that the mortuary temple may have been a stone representation of the royal palace at Memphis. Whereas Zoser had a large stone-built Heb-sed court to the east of his pyramid, the later kings were content with low reliefs of the Heb-sed ceremony, placed on the walls of the upper temple of the pyramid (p. 63). In dealing with the early stone pyramids, emphasis is carefully laid upon the smallness of the blocks used (pp. 63-4); not only did they approximate in size to the mud bricks, but they were often likewise laid in headers and stretchers. The casing blocks were often inclined in order to minimize the labour involved in trimming them *in situ* (p. 69). The evidence he adduces to show that the Maidum pyramid of Snefru was never completed tends to receive confirmation (if any were needed) from the recent excavation of Snefru's double-angled pyramid at Dahshur, the details of which affect the remarks on pp. 83-84 and 239-240 of Mr Edwards' book, which was too far advanced to include more than a postscript relating to this excavation. It is likely that Snefru built three pyramids—that at Maidum and both the stone pyramids at Dahshur, as Alexandre Varille has recently suggested (*A propos des Pyramides de Snefrou*, Cairo, October 1947).

ANTIQUITY

Comparison with the Dahshur pyramids suggests that the horizontal passage leading south from the lower chamber of the pyramid of Kheops was intended to lead to another chamber which was never cut (p. 89). Mr Edwards emphasizes the probable rôle of the Sphinx as a representation of Khephren in the form of the Sun-god acting as the guardian of the Giza necropolis (p. 107). His account of the Opening of the Mouth ceremony before the statues in the funerary temples is illuminating (p. 111), as are also his remarks on the duties of the pyramid priesthoods and the privileges enjoyed by them (pp. 124, 146). The author has made extensive use of his study of the reliefs on the walls of the causeways and funerary temples, and by their aid has gone a long way towards interpreting the functions of the pyramid complex (see especially pp. 158-161).

A word may be said as to the author's terminology of the pyramid temples. He follows normal usage in referring to the temples adjoining pyramids as Mortuary Temples, and he refers to the structures at the lower end of the causeways as Valley Buildings. The valley structure of Mycerinus seems definitely to have served as a funerary temple, as statues, stone vessels, and implements for use in the Opening of the Mouth ceremony were found therein. The temple adjoining the pyramid of Mernere seems to have been called *šꜣꜣꜣ hrt*=the upper room or temple, in the contemporary text of Uni (line 41), and in view of this there seems no reason why these two types of building should not be called the Upper and Lower Temples. As time went on the Upper Temples seem to have grown in importance at the expense of the Lower Temples, hardly any traces of which have yet been found at Middle Kingdom pyramid sites.

In a final chapter, on the construction and purpose of the pyramids, Mr Edwards throws a good deal of light on how they were built. He describes, *inter alia*, the factors which governed the choice of a site; how the levelling of the pyramid area may have been assisted by flooding; and how the four sides were oriented to face the cardinal points. He makes (p. 227) the valuable suggestion that the first course of casing may have been laid before the pyramid core, in order to define the size and orientation of the structure. After describing the methods of quarrying limestone and granite he deals with the means of transporting the blocks to the sites of the pyramids. There can be little doubt that the granite from Aswan was transported downstream during the inundation period when the boats were less likely to run aground. The existence of built causeways for facilitating transport of the stone from the quarries to the Nile might have been stressed. The main problem, how were the stones lifted to form the various courses of masonry, remains essentially unsolved. That some kind of construction ramps of mud-brick were used is clear, but the exact form of those construction ramps remains in doubt. The two lines of masonry extending south-west of the north stone pyramid of Dahshur are, in the reviewer's opinion, the remains of limestone causeways from a local limestone quarry. If they were remains of brick construction ramps, as Mr Edwards suggests (p. 221), they would show much darker on air photographs. He rightly insists that, with the possible exception of the lowest course, the casing stones were dressed after being placed in position, despite Petrie's arguments to the contrary (pp. 225-7).

The author's final pages on the symbolic meaning of pyramids, based partly on a comparative study of the ancient Egyptian words for 'to ascend' and 'pyramid', are valuable. There can, it seems, be little doubt that the original stepped form of pyramid symbolized the deceased king's stairway to the sky, referred to in the Pyramid Texts.

The line-drawings by J. Cruikshank Rose are excellent, but one rather wishes the plans were accompanied by scales. The photographs include various statues of pyramid builders, and an excellent oblique aerial view of the pyramids of Giza. In short, the author is to be congratulated on a fine piece of work.

L. V. GRINSELL.

REVIEWS

EARLY INDIAN PAINTED POTTERY. By D. H. GORDON. *Journ. Indian Soc. Oriental Art*, XIII (1945), 35.

In this study Col. Gordon draws attention to the danger of thinking in terms of a 'Painted Pottery Period' in Indian prehistory, since the technique of pottery painting has lasted little changed in Northwest India until the present day. Such an idea has long been exploded in Europe, and would hardly be held by reputable Oriental scholars even in India, but the warning may be valuable to the amateur. Gordon refers to the late Brig. Ross's invaluable work at Rana Ghundai (now published in *Journ. Near Eastern Studies* v (1946), 284-316), but seems to under-rate the Buff Ware and Red Ware grouping of Iranian prehistoric wares and their Indian counterparts—a broad distinction which we owe to McCown and which seems to the reviewer only not the only reasonable basis for classification, but one which really does make sense in the light of increasing knowledge from India. The comparisons between Hissar I and Rana Ghundai II have been considerably strengthened by the new material published by Ross, and the 'archaistic' wares of Bampur and Khurab cited by Gordon in this connexion are not really relevant, their affinities being with those of Shahi-tump, a site which can hardly be earlier than Akkadian. For the Nineveh v—Tell Billah chalices with animal friezes quoted by Gordon, McCown's remarks should be noted (*Comparative Stratigraphy of Early Iran* (1942), p. 48, note 88).

The survival of painted pottery traditions in Western India is a matter of great interest: at Ahichhatra the fine black-on-grey painted wares seem likely to be 4th century B.C., and Sassanian textile patterns were copied in the 6th-7th centuries A.D. by pot-painters in the Quetta region, which gives an approximate dating-point in an otherwise rather vaguely 'post-prehistoric' series. But after all, in Sind, as the late Ernest Mackay pointed out, pottery painted with good Harappa Culture designs was being made a few years ago, so we may have to be more than usually chary about making chronological equations in a country so conservative as India. STUART PIGGOTT.

ALEXANDER THE GREAT. By W. W. TARN. *Cambridge University Press*, 1948; 2 vols., 'Narrative', pp. xi+161, with a Map, 10s 6d; and 'Sources and Studies', pp. xiii+477, with a Map, 30s.

If it be true that the great deserve and indeed require to live afresh by the pen of each successive age, then Alexander above all requires it, for every age so far, since the study of Greek history was reborn with Niebuhr, has had not its own Alexander but its Alexanders. Dr Tarn in his chapters in the *Cambridge Ancient History* (vol. VI, 1927) had re-created what was to many perhaps the most satisfying Alexander hitherto, and now in these two volumes he both gives his latest thoughts on the subject and explains, far more intimately even than Beloch ever explained to his readers, just why it seems to him that Alexander was like this, rather than like that, or like the other.

The 'Narrative' might appear to the casual reader almost identical with the text of the chapters in the *Cambridge Ancient History*, but in fact it contains a large number of changes mostly small if reckoned by a count of words or phrases, but often really important. Thus though Dr Tarn's 'general view of Alexander remains unchanged' (Preface), his views are amplified, modified, or occasionally even reversed on such important matters as the status of the liberated Greek cities in Asia Minor (pp. 31 ff.), the battles of Gaugamela (pp. 46 ff.) and the Hydaspes (p. 95—where the new sentence inserted in the text at ll. 18-23 must surely be the most illuminating remark yet made about this battle by any writer ancient or modern), Callisthenes and the 'Son of Zeus' legend (p. 78), the

ANTIQUITY

'new army' of the far-eastern and Indian campaigns (pp. 82 ff. and 92 f.), the celebrated banquet at Opis (pp. 116 and 147), the alleged plans for world conquest (pp. 121 f.), the satrapal system in the empire (p. 127), the Alexandrias and the new city-foundations in general (pp. 127, 133, 136), Cleomenes in Egypt (pp. 129 f.), the policy of race fusion (p. 137). Those are the most important topics of which Dr Tarn finds something new to say: and the most important of these in their turn are the subject of essays in the volume entitled 'Sources and Studies'.

For our knowledge of the thirteen years of Alexander's reign we owe surprisingly little to the archaeologist, though a few important inscriptions and very many coins have been preserved and interpreted. Nor is it likely that archaeology in the future will be able to add more than here and there some useful detail to the picture—as for example (we may hope) from excavations such as those now or recently in progress on the site of Alexandria of the Caucasus (as the Greeks called the Hindu Kush). But for the greatest truths about these years so decisive in human history we must always rely mainly on a literary tradition of an almost unparalleled variety (in so small a compass), and it is by what he makes of this tradition that every historian of Alexander will ultimately stand or fall. Dr Tarn has lived with his sources for a good many years now, and knows them in a way which puts even some good historians to shame, and bad ones to the blush. His work here in 'Sources and Studies', especially Part One (the so-called 'Vulgate' and its sources) and Appendices 15, 16, 17, 22, 23 and 24, represents incomparably the greatest achievement in this field up to this time, and in particular his revaluation of Cleitarchus and Aristobulus, though it is sure to be contested, will be found ultimately, I am convinced, to have placed the study of Alexander on a sounder basis than ever before.

Students of the Hellenistic world owe very much already to Dr Tarn, and this splendid book greatly increases their debt. G. T. GRIFFITH.

VERGIL'S LATIUM. By BERTHA TILLY, PH.D., M.A. (Lond.). pp. xv, 123 with 36 photographic plates and 8 maps or plans; Basil Blackwell, Oxford, 1947. 15s. net.

Dr Tilly's topographical study of the Roman Campagna is a companion volume to the last six books of the Aeneid. In it she describes the Campagna as it is today, or as it was when her researches were carried out in the years between 1933 and 1939, for the area lay in the track of the allied armies in their advance on Rome from the Anzio beach-head, and some of the remains which she illustrates may have suffered damage or destruction. Dr Tilly gathers historical, archaeological and topographical material in an attempt to reconstruct the area as it was in Vergil's day and to relate the epic narrative to the actual countryside in which it is set.

Dr Tilly observes that 'archaeology does not confirm the tradition that either Ardea or Lavinium was an inhabited site in Trojan times, or that they could, in reality, have played any part in the Aeneas-legend'. Nevertheless the study of the ancient places, already in Vergil's own day a decaying region, which he chose for the scene of the great climax of his poem, has had a fascination for scholars, and Dr Tilly follows in the footsteps of a number of distinguished predecessors in the task of reconstructing them as Vergil knew them. 'Totius Italiae curiosissimum fuisse Vergilium multifariam apparet', says Servius in a comment, which the author quotes on her title page, and scraps of information on local cults or local history may often throw light on Vergil's thought. Thus Strabo's record of a temple sacred to Venus near Ardea, where the Latins held federal cults, and ceremonies were observed said to date back to the time

REVIEWS

of Aeneas, may well supply the link between the religious importance of the city as a cult centre and its prominence in Vergil's narrative. Dr Tilly has an interesting chapter on the vexed question of Vergil's Laurentum, for which she proposes a new location, rejecting the theory of Dessau and Carcopino, accepted by many scholars, 'that Lavinium alone existed and bore the name of Laurentum until she was founded for a second time by Aeneas, when he married Lavinia and took over the government from Latinus'.

Under the title 'The Trojan landing and Troia Nova' the author discusses Ostia in Vergil's day in the light of modern investigations both of the city itself and of the considerable physical changes in the area of the Tiber's mouth, and suggests reasons for the abandonment by Vergil of the earlier tradition that Aeneas landed at a point further south. Other chapters deal with topographical problems connected with the river Numicus, the oracle of Faunus at Albunea, and the ager Solonius. The book is a useful introduction to this aspect of Vergilian studies and contains much that is of interest and value. Some blemishes may be noted. The illustrations from Dr Tilly's own photographs are plentiful and mostly good, but are often remote from the corresponding text and sometimes inadequately described, e.g. 'Ostia' might be added on plates 9-12. Either the map on page 5 or its title is upside down. In the first line of the quotation from Vergil (Aen. VII, 109-11) on page 26 the omission of -que destroys the scansion, and in the passage from Ovid (Met. XIV, 573-7) the misprinted text 'congerie et media' for 'congerie e media' mars the sense and the grammar. On page 65 by beginning her quotation from Lucan in the middle of a sentence and omitting a comma after 'penates' Dr Tilly obscures the sense and the syntax. In compensation the insertion of a comma after 'fines' in the second line of the quotation at the head of chapter VII (Aen. XI, 316-21) has the same effect. To say, as the author does in footnote 4, page 44, that 'Vergil's words in Aen. VII, 412, "dictus avis" might be taken to mean "called (by the name of) a bird"' revives an old and highly fanciful view and ignores the quantity. The carelessly worded statement on page 3 that 'for Vergil, however, the Trojans first come to land in Italy on the banks of the Tiber' might suggest that the author had forgotten the famous opening of Book VI. In spite of such minor lapses the book may be recommended as a very useful supplement to purely verbal commentaries on Vergil's text.

G.F.F.

Correspondence

Sir,

I recently attended the 29th International Congress of Americanists in New York, during which I heard some information of interest in connexion with your Editorial Notes and my review-article 'Peru before Pizarro' in your September issue.

When I first heard of it earlier in the year I hoped that radiocarbon might at last provide us with some reliable dates for South American archaeology, and the Congress proved that American archaeologists are fully alive to this possibility. We had the privilege of hearing a lecture by Dr Libby on radiocarbon dating, and he has applied the method to some wood collected by Junius Bird at the base of the Cupisnique horizon in Peru. The figure he got was about 800 B.C. with a possible error of 200 years either way. He was very cautious about giving figures at this stage, but he felt that the result he got here was reliable enough to quote. Readers of my article will recall that the date falls well enough between Bird's estimate of 1800 B.C. and previous guesses of about A.D. 1.

I am, Sir, your obedient servant

G. H. S. BUSHNELL, *Curator*

University Museum of Archaeology and Ethnology, Cambridge

5 November, 1949

Index

- Abbeville, 116 ff.
 Addison, F., 51
 Adgefrin, 211
 Adi Caieh, 193
 Aeneas Pontifex, 53-4
 Air-photographs, 58 ff., 109
 Aisled round-house, The, 107
 Aitchison, Sir Walter de L., 212
 Albery, Mr William, 42
 Alexander the Great, 227-8
 Allen, Major, 59, 210
 Alphabet, 108
 'Ancient India', 107
 Anthropology, principles of, 54-6
 Apulia (neolithic), 61
 Arbor Low, 33, 38-9
 Archiac, 118
 Aregawi, 198
 Armillas, Pedro, 137
 Arthur's battles, 48-9, 107
 Astrology, 223
 Aufrère, 116 ff.
 Axum, 188, 190, 193
 Azarbaijan, 47-8
 Aztecs, 56
 Balba, 27
 Banba, 27
 Baradez, J., 169, 206
 Bark cloth (Peru), 136
 Bark, spools of, 207
 Beaulieu barrows, 39
 Beck (beads), 201
 Becker, C. J., 129 ff.
 Bennett, W. C., 137
 Bird, Junius, 136, 229
 Blewburton Hill (Berks), 208-11
 BRADFORD, JOHN; 'Buried landscapes' in
 Southern Italy, 58-72
 Branton (Devon), 181 ff.
 Breguoin, Arthur's battle of, 48-9
 Bremenium, 49
 British Art and the Mediterranean, 165-6
 Brixham, 120 (cave), 185
 BROWN, T. BURTON; Recent discoveries in
 Azarbaijan, 47-8
 Buckland Rings (Hants), 211
 Burke, the late Mr Thomas, 40
 Bur-Sin, 93
 BUSHNELL, G. H. S.; Peru before Pizarro, 136-9
 BUTLER, HUBERT; The Dumb and the Stam-
 merers in Early Irish History, 20-31
 Buxton, D. R., 188, 200
 Cairn Holy, 170
 Cairnpapple, excavations, 32-9
 Canterbury Excavations, 1944-8, 153-60
 Cartularies, 184, 224
 Castel del Monte, 70
 Cave-man (France), 44-5
 Chapple and Coon, 54
 Chavin culture, 137
 Cheviot hill-fort, 45-6
 Cheviots, 212
 Chicama valley, 136
 CHILDE, V. G.; The Origin of Neolithic
 Culture in Northern Europe, 129-35
 Childe, Gordon, 103, 106
 Cholsey Hill, 208
 CLARK, GRAHAME; Urn-fields and vital
 statistics, 46-7
 Clark, Dr Grahame, 207-8
 Clifton, 185
 Clyst, Broad, 185
 Collins, Mr A. E. P., 208, 210
 Council for British Archaeology, 73
 Court Leet (Portland), 143
 CRAWFORD, O. G. S.; Men, Machines and
 History, 100-6
 Crawford, O. G. S., 188
 Crèveœur, J. B. de, 115
 Crock Cleugh (Roxb.), 212
 Crusades, 83
 Cupisnique sherds, 137
 Curwen, E. Cecil, 53
 Danish archaeology, 53
 Darby, Professor, 180
 Dark Ages, 78-82
 'Dartians', 127
 Debra Damo (Ethiopia), 188-200
 Deschamps, 84, 91
 Dessi, 23, 25
 Devonshire, The Open Field in, 180-7
 Dog's paw, monastery called, 25
 DOUCH, ROBERT; Customs and Traditions of
 the Isle of Portland, Dorset, 140-52
 Dunstable (Long Barrows), 219-20
 Durrów, Book of, 111
 Dysser, Danish, 129
 Ecuador, 137
 Edwards, I. E. S., 95
 Edwin, King, 211
 Emery, Walter B., 50
 Erbil, 222
 Eridu, 93
 Ermington, 185

INDEX

- Ethiopia (Debra Damo), 188-200
 EVANS, JOAN; Ninety Years Ago, 115-25
 Evans, John, 121 ff.
 Faeroes, The, 53
 Faience beads, 201-5
 Falconer, Hugh, 120
 Falla, R. A., 176
 Farmers, First Northern, 134
 Field-names quoted, 181, 183-4
 Field-work, scope for, 2, 30
 Fields, Roman, 65
 Flood, The, 119
 Folsom man, 136
 Fontéchevade man, 127
 Food vessel, 36
 Forbès, H. O., 176
 FRERE, SHEPPARD; Canterbury Excavations, 1944-8, 153-60
 Galley Hill man disposed of, 127
 Geography, ancient, 107
 Geoy Tepe, 47
 Glastonbury Chartulary, 224
 Goidels, 20
 Graig Lwyd, 33
 Gray, H. L., 180
 Great Man, the, 102 ff.
 Griffith, G. T., 228
 Grimes, W. F., 206
 Grinsell, L. V., 226
 Group theory, 126
 Harappa, 166-7, 201-5
 Hawaiki, 172
 Hawkes, C. F. C., 206, 210
 Hebrew scrolls, 169
 Henge, 32
 History, the new, 101 ff.
 History of Science (Congress), 214
 History, Universal, 100 ff.
 Hocart, 98
 Hogg, A. H. A., 214
 Hooke, S. H., 110
 HOOTON, E. A.; Human Evolution, a Review, 126-8
 Horsham, 42-3
 Houses, Aisled Round, 107
 Neolithic (plans), 63
 Polish, 132
 Hownam Rings, 45
 Human evolution, 126-8
 Hutchinson, R. W., 52, 220
 Iceland; Roman coins found in, 161-3
 Icknield Way, the, 208
 Ikhnaton, 105
 Indus civilizations, 166-7
 'Irish' art, 110
 Irish history, Early, 20-30
 Italy (air-photos), 58-72
 JACKSON, KENNETH; Arthur's battle of Breguoin, 48-9
 Kaikoura, 174
 Keith, Sir Arthur, 126-8
 'Kimberlin', 140
 KING, D. J. CATHCART; The taking of Le Krak des Chevaliers in 1271, 83-92
 Kingship and the Gods, 93-9
 Knossos, 203
 Kollman, Paul, 215
 Krak des Chevaliers, 83, 85 (plan)
 Kroeber, 137
 Kubler, George, 139
 Ladle Hill, 211
 Leicester, history in, 112
 Jewry Wall, 107
 Lethbridge, T. C., 162-3, 169
 Libby, Prof. W. F., 113, 229
 Lilley, S., 101 ff.
 Long barrows, 129, 133 (plan), 135, 218-20
 Longman's Hill, 218
 Lothian, West, 32 ff.
 Lucera, 66-7
 Lullingstone, Roman villa, 170
 Maglemosian, 207
 MALLOWAN, M. E. L.; Kingship and the Gods: a Review, 93-9
 Maoris, 172 ff.
 Marriage-customs (Portland), 146, 149 ff.
 Marshall, Sir John, 17-18
 Marxism, 40, 106
 Meare lake village, 107
 Medieval landscape (Italy), 70
 Men, machines and history, 100-6
 Menchecourt, 119
 Menes, 94
 Mesolithic, 113, 207
 Mexico, 138
 Moas and Man, 172-9
 Mohenjo-daro, 201
 Moore, John W., 207
 Moulin Quignon jaw, 124
 MOVIUS, HALLAM L., JR.; Caveman in France, 44-5
 Mundy, Peter, 152
 Nennius, 48-9
 Neolithic culture, origin of, 129
 communities (Italy), 60
 New Zealand, 172 ff., 223
 Nicknames (Portland), 142
 Northumberland, 211-14

ANTIQUITY

- Nubian Treasure, 50-1
 Ordnance maps, 2, 170
 Orwins, the, 180
 Ossory, 23, 28-30
 P-Celts, 20
Pachyornis, 176
 'Past and Present' series, 103
 Paulinus, 211
 Persepolis, 107
 Perthes, Boucher de, 115 ff., 177
 Phillips, C. W., 170, 219
 Picard, Dr Casimir, 116
 PIGGOTT, STUART; The Excavations at Cairn-
 papple Hill, West Lothian, 1947-8, 32-9
 Piggott, Stuart, Prof., 168, 170, 204, 220, 227
 Pins, 220
 Pinsard, M., 121
 Pitsford (Long Barrow), 218-20
 Pitt-Rivers, General, 2, 170, 206
 Pitt-Rivers museum, 58
 Pizarro, 136
 Poidebard, the Rev. Father, 169
 Portland, Isle of, 140-52
 Prestwich, Joseph, 120 ff.
 Problems and Policies, 73-92
 Professionals and Amateurs, 1-3
 Pyramids, 95, 225-6
 Q-Celts, 20
 Quarley Hill, 211
 Quarrying customs, 147 ff.
 Quignon, Moulin, 124
 Radford, Mr Raleigh, 217
 Radiocarbon, 113-4, 229
 Reeve (Portland), 143
 Reeve staffs, 144-5
 Richmond, I. A., 108
 Rigollot, 120
 Rimush of Agade, 93
 Ritual and planning, 194
 Roman ways, 107
 Romano-Buddhist Art, 4-19
 Rose, H. J., 53
 Rowse, A. L., 187
 Saladin, 84
 San Lorenzo, 71
 Sark, 3
 Scanlan, 26-7
 Scotland, Roman, 108
 Scott, Sir Lindsay, 170
 Shaddad, Ibn, 88-90
 Shah Tepe, 47
 SHETELIG, HAAKON; Roman coins found in
 Iceland, 161-3
 Shipley Hill (Leics.), 220
 Silsbury Hill (Berks), 208
 Slee, A. H., 186
 Smallcombe, W. A., 208
 Smith, C. Roach, 118
 Sneideru, 114
 Southampton, Saxon, 101
 St. Canice, 28-9
 St. Columba, 28-9
 St. George's St., Canterbury, 156-7
 St. Joseph, Dr, 169
 St. Kieran, 22
 St. Moling, 29-30
 Stalin, Marshal, 106
 Steward, J. H., 137
 Stone, J. F. S., 216
 Stonehenge, 33
 Strip-cultivation, 3
 Strong, Duncan, 137
 Stukeley, William, 218
 'Sumer', 107
 Tammuz, 97
 Tancred, 83
 Tavistock Abbey, 182
 Taxila, 4 ff.
 Tayacian, 44
 TAYLOR, E. G. R.; Roots and Origins, a
 Review, 40-3
 Technology, 101
 Tilly, Bertha, 228
 Totemism, 56
 Tozzer and magic, 55
 Urmia, Lake, 47
 Urn-fields, 46-7
 Virgil, 53-4, 228-9
 Viru valley, 136-7
 WAINWRIGHT, F. T.; Problems and Policies,
 73-82
 Wainwright, F. T., 73, 112, 225
 Wessex, 210
 WHEELER, R. E. M.; Romano-Buddhist art,
 4-19
 White, Leslie, 102 ff.
 Whithorn, Excavations at, 217-8
 Widegren, 98
 Wood (dating by), 113, 229
 Woodbury, Little, 211
 Wright, Thomas, 119
 Yeaving, Earthwork at Old, 211, 213 (plan)
 Zabanbur, 193
 Zimbabwe, 206
 Zoroastrianism, 94
 Zoser, 114